ESSAYS ON LABOR AND HEALTH ECONOMICS

A Dissertation

Presented to the Faculty of the Graduate School

of Cornell University

in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy

By

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May 2013

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ESSAYS ON LABOR AND HEALTH ECONOMICS

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Cornell University 2013

The first essay looks how the disability wage gap as well as the gender, race, and ethnicity wage gaps are affected by macroeconomic conditions. Even though a large literature looks at the trends of these wage gaps, very little research considers their cyclicality. I use the SIPP linked to administrative earnings records to look at how these gaps vary with local labor market conditions from 1978 to 2010. For annual earnings, the disabled and blacks seem to fare better than their counterparts as labor market conditions worsen while women seem to fare worse than men, and the results are mixed for Hispanics. For hourly earnings, the results are largely mixed and inconclusive. There is also evidence that these results vary by decade.

The second essay asks whether the gender gap in total compensation is smaller than the gender wage gap. One potential explanation for the observed gender wage gap is that men and women value the nonwage aspects of a job differently. I construct two individual level measures of total compensation – one using supplemental CPS data on employer contribution to health insurance premiums and one using the NLSY linked to employer cost data. I find that the observed gender gap resulting from these measures of total compensation is almost identical to the observed gender gap in wages.

The third essay considers how parents allocate scarce resources among children with different levels of initial endowment. Parents that are interested in maximizing the return on their investment might reinforce initial conditions, but parents motivated by equity might compensate. I use the SIPP to directly measured health endowment as whether the child has any health conditions and parental investment as the frequency with which parents do various activities with each child.

The results show that there is some evidence that parents do not invest equally in children of different health endowments, but the evidence is far from overwhelming. Moreover, the results differ depending on parents' education and the children's age group. In general, these results seem to indicate that pattern of parental behavior depends crucially on the specific investment.

BIOGRAPHICAL SKETCH

Chen is from the great state of California. She has also resided in China, Oklahoma, Maryland, Washington D.C, Boston, and New York City. She received her Ph.D. in economics from Cornell University in 2013. Prior to graduate school, she studied economics and brain and cognitive sciences at the Massachusetts Institute of Technology and worked at the Council of Economics Advisers. She will be joining the New York City office of Analysis Group, Inc. after graduation.

To my parents.

ACKNOWLEDGMENTS

I would like to thank my advisers – John Abowd, John Cawley, and Kevin Hallock – for their support and guidance over the past few years. Also, I want to thank the many people at the Census Bureau's Center for Economic Studies and the Social, Economic, and Housing Statistics Division as well as the staff at CISER who helped to facilitate much of this research.

I was fortunate enough to go through this experience in the company of great friends. I want to thank all of you who made my stay in Ithaca much better than it could have been, especially those who toiled beside me at the RDC.

I want to thank my parents for being the best parents in the whole world. I checked. They really are. And lastly, I want to thank Kris for his love, patience, and willingness to ride the bus.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

LIST OF CHARTS

CHAPTER 1

WAGE GAPS OVER THE BUSINESS CYCLE:
Disability, Gender, Race, and Ethnicity

Chen Zhao[1]

ABSTRACT

A large literature documents how the disability wage gap, as well as the gender, race, and ethnicity wage gaps, have changed over time, but there is little research looking at how these wage gaps are affected by macroeconomic conditions. Due to discrimination or for compositional reasons, the disabled, women, blacks, and Hispanics could well be more adversely affected by economic downturns than their counterparts. These groups may also fare better in adverse labor market conditions because of, again, compositional reasons or because of selection into particular industries or occupations that are less cyclical. I use restricted access SIPP data that has been linked to administrative earnings records from the Social Security Administration to look at how the gap in earnings and wages faced by individuals with various types of disabilities as well as women, blacks, and Hispanics varies with the level of the local unemployment rate from 1978 to 2010. For annual earnings, the disabled and blacks seem to fare better than their counterparts as labor market

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I am grateful to John Abowd, Richard Burkhauser, John Cawley, Shooshan Danagoulian, Bill Erickson, Kevin Hallock, Arun Karpur, Martha Stinson, and Michael Strain as well as participants in the Health Economics, Health Behaviors and Disparities lunch and the Labor Economics/Policy Analysis and Management seminar at Cornell University for their helpful comments.
Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed.

conditions worsen while women seem to fare worse than men, and the results are mixed for Hispanics. For hourly earnings, the results are largely mixed and inconclusive. There is also evidence that these results vary by decade. For example, in the 1980s and 1990s, the disabled seem to fare downturns better than the non-disabled, but for the 2000s, the opposite appears to be true.

1. INTRODUCTION

The economic disadvantage faced by people with disabilities and the wage/earnings gap faced by women, blacks, and Hispanics are well established in the literature (Altonji and Blank, 1999; Haveman and Wolfe, 2000; Fryer, 2011). Of course, being disabled is very different than being female, black, or Hispanic since the former can be time variant and is, therefore, much more difficult to define. Nevertheless, all of these groups – the disabled, women, blacks, and Hispanics – can be thought of as being somewhat "economically vulnerable" and this paper explores how their relative well-being in terms of earnings has changed over the course of the business cycle in the five most recent recessions in the US using linked administrative and survey data spanning the period 1978 to 2010.

The labor market consequences of disability are well documented. Not only do the disabled have a weaker attachment to the workforce and work fewer hours, they also earn lower wages than the non-disabled. Haveman and Wolfe (1990) find that the ratio of the earnings of the disabled to the non-disabled ranges from just over half to three-quarters between 1962 and 1984; Burkhauser et al (2001) finds a steady decline in this ratio over the 1990s. One of the main reasons for these results is that the disabled as a group have much less human capital. As a result of the adverse labor market consequences of disability, the disabled are much more likely than their non-disabled counterparts to be living in poverty (Haveman and Wolfe, 2000).

The labor economics literature has also long documented a persistent wage gap faced by certain demographic groups, including women, blacks, and Hispanics. Numerous cross-sectional studies have looked at both the levels and trends of the wage gaps between these groups and their counterparts. Of course, in any study that measures wage gaps, the marginal product of each worker is usually not observed and thus, interpreting the wage gap or searching for a casual mechanism is a much more difficult exercise than the simple measurement. Explanations that are often put forth to explain these wage gaps between demographic groups include differences in human capital, different preferences, differences in non-cognitive skills and discrimination (Antonji and Blank, 1999; Bertrand, 2010).

While the levels and general trends of the gender, race, ethnicity, and disability wage gaps are been explored at length, very few studies have looked at the cyclicality of these wage gaps and how they change as local labor market conditions change. The empirical evidence would especially be of interest since economic theory does not have a clear prediction for the direction of the effect. Possible self selection among the more "vulnerable" groups (women, blacks, Hispanics, and the disabled) into industries and occupations that are more shielded from cycle effects would lead to smaller earnings gaps at higher unemployment rates. However, employers may have greater ability to engage in taste-based discriminate during economic downturns since the perceived cost of discrimination may be greater in a tighter labor market, leading to larger earnings gaps as labor market conditions worsen. Compositional shifts as labor market conditions change could also impact earnings gaps in either direction depending on whether better or worse workers in each group are more likely to exit the labor force during economic downturns. In addition, for the disability comparisons, the population that self-identifies as being disabled may change as the unemployment rate changes and also affect earnings gaps depending on whether better or worse workers are more likely to self-identify as being disabled as labor market conditions deteriorate.

Among the few studies to have looked at this question is recent research by Biddle and Hamermesh (2012), which finds evidence that women, Hispanics, and bad-looking workers experience greater earnings disadvantages during cyclical downturns and after experiencing negative industry-specific demand shocks. These findings suggest that, perhaps, the disabled could also be more negatively affected by adverse labor market conditions relative to the non-disabled and invite research looking at the cyclicality of the disabled/non-disabled earnings ratio as well as the demographic wage gaps that Biddle and Hamermesh focus on.

The literature on how the disabled fare in recessions compared to the non-disabled is especially thin. While there are hints in the literature that non-white disabled men in particular and perhaps, all disabled men, are disproportionately negatively affected by recessions, there are no in-depth studies looking at these effects, particularly as they relate to the more recent economic downturns. (Haveman and Wolfe, 1990; Daly, 1994; Burkhauser, et al, 2001)

While this study considers the disability wage gap as well as the gender, race, and ethnicity wage gaps, there is reason to think that the cyclical behavior of the disability wage gap might differ from that for demographic wage gaps. As previously touched on, the disabled are different from the other economically vulnerable groups for two important reasons. First, disability is much more complex than gender, race, ethnicity, and even physical appearance. There exists no obvious definition of disability and who should be included in each group when comparing the disabled to the non-disabled is difficult to determine. This ambiguity makes estimation difficult as the relevant effects will change as definitions and, therefore, relevant samples change.

The second difference is that, as a group, the disabled have social insurance programs that attempt to ameliorate the economic disadvantages they face, which in turn may affect how questions about disability status are answered in surveys. These social programs also affect incentives to join the labor force for those whose disabilities do not prevent working. A large strand of literature documents the recent increase in SSDI rolls and the corresponding decline in labor force participation among the disabled. (Bound and Burkhauser, 1999) In the context of this study, the effect of these incentives may well be exacerbated during recessions. Someone who may qualify for Social Security Disability Insurance (SSDI) or Supplemental Security Income (SSI) programs may hold a job when jobs are plentiful, but may not look too hard when labor market conditions deteriorate. As Haveman and Wolfe touched on this in their *Handbook of Labor Economics* chapter, "[disabled] workers… are also more likely to experience lower earnings in a recession, leading to withdrawal from the work force if disability benefits provide a floor on the reservation wage of disabled workers."

This study uses a unique linked dataset, the SIPP Gold Standard File (GSF), to estimate the effect of local unemployment conditions on the disability, gender, race, and ethnicity wage gaps. The GSF links nine panels of the SIPP (Survey of Income and Program Participation), conducted between 1984 and 2010, to the Social Security Administration's earnings and benefit records from 1978 to 2010[2]. This data allows for the people surveyed in the nine SIPP panels, which normally span 2 to 4 years, to be observed (in terms of their earnings and receipt of benefits) for 32 years. During

[2] The Summary Earnings Records date back to 1951 but the Detailed Earnings Records only go back to 1978. See the data section for more information.

this 32-year period, there were five recessions, as determined by the NBER (National Bureau of Economic Research).[3]

Because of its unique structure and length, the SIPP GSF allows for a more in-depth analysis of this question than the dataset used by similar studies. These data allow me to follow disabled and non-disabled respondents of the SIPP longitudinally over the course of four (or five depending on how they are counted) business cycles, which are preferable to the cross sectional data used by previous studies that have looked at trends in the disability earnings gap. Also, the SIPP is better than the CPS (Current Population Survey) or ACS (American Community Survey), which is often used in these types of studies, because it asks detailed questions about functional limitations, conditions, and work limitations as opposed to simply asking about work limitations. This captures much more of and, perhaps, a different part of the population that is disabled since how a person answers the work limitation question may be especially swayed by a person's specific conditions that are not related to disability. For example, Burkhauser, et al, 2010 shows that, compared to the working impaired, a much larger percentage of the non-working impaired reports being work limited.[4]

Finally, since I know the state the person lives in during their time in the SIPP I am able to exploit state level variation in labor market conditions. State level unemployment is a much finer measure of the labor market conditions that a person is facing compared to the national unemployment rate since at any given time, there is a substantial amount of variation in state-level unemployment rates.

[3] The peaks are Jan 1980, July 1981, July 1990, March 2001, and December 2007. The troughs are July 1980, Nov 1982, March 1991, Nov 2001, and June 2009. (http://www.nber.org/cycles.html)
[4] A conjecture about why this is could be is that, for example, a disabled person who is employed may not feel limited in the kind of work he can do since he is working while a disabled person who is not employed is more likely to feel work limited since he has not been able to find work.

My results show that the gap in annual earnings between the disabled (for both those with work limiting disabilities and those with difficulties and limitations) and the non-disabled shrinks as the labor market conditions worsen. This also appears to be true for blacks when only considering years with positive earnings (the opposite is true when including years of zero earnings). In contrast, my results indicate that the gender gap in annual earnings increases as the unemployment rate increases. The results for Hispanics is largely inconclusive as are the results for hourly earnings, but however, some specifications do suggest that the Hispanic-white gap increases with the unemployment rate. I also look at how these effects vary by business cycles and there is significant heterogeneity over time.

The rest of this paper proceeds as follows – section two will discuss the literature on labor market outcomes for the disabled, the literature on the gender, race, and ethnicity wage gaps and how I will measure disability, section three will discuss in detail the restricted access data that I use, section four presents the estimation techniques, section five gives the results, and the last section provides a discussion.

2. LITERATURE AND BACKGROUND ON MEASURING DISABILITY

2.1 Related Literature on the Disabled

Numerous studies have documented the earnings and employment trends of the disabled. Two general trends emerge – the disabled face a large employment and earnings gap relative to the non-disabled and the employment rate of the disabled has declined steadily over the 1990s and 2000s (Burkhauser, et al, 2001, 2010). Much of this decline has been associated with the increase in SSDI and SSI rolls since the 1990s and at least some of the increase in SSDI and SSI program participation can be attributed to a relaxation of eligibility requirements and an increase in income replacement rates. (Bound and Burkhauser, 1999; Autor and Duggan, 2003)

The employment rate among disabled working-age men has been less than half of that of non-disabled working-age men since the early 1980s and has declined steadily since the 1990s to be almost one-fifth of that of their non-disabled counterparts (Burkhauser, et al 2010). Among working-age women, the employment ratio of the disabled is also much less than half of that of non-disabled women, but it has been increasing steadily through the 1980s and 1990s from 28.5% to 33.3% by 1999 (Burkhauser, et al 2001).

Both disabled men and women also have earnings that are far below those of non-disabled men and women. The income of disabled men has declined from about 60% to slightly less than 50% of that of non-disabled men from the early 1980s. Disabled women's income has also experienced the same decline from about 60% to about 50% of that of non-disabled women. (Burkhauser, et al 2001)

The existing research on how the labor market outcomes of the disabled fare over the business cycle relative to their non-disabled counterparts is much thinner. Daly (2004) finds evidence of strong cyclical patterns and that, for African American men, recessions have particularly negative effects on labor earnings over the 1970s and 1980s. This study is limited in that she only looks at disabled/non-disabled earnings ratios and also in that she looks at a very limited population. Haveman and Wolfe (1990) look at the economic well being of the disabled from 1962 to 1984 and find especially strong negative effects from the recession of 1982 on the relative wellbeing of the disabled. This study also only looks at earnings ratios and does not employ more rigorous regression analysis. While these studies hint at cyclical effects on the relative wellbeing of the disabled and that adverse labor market conditions are particularly tough for the disabled, a more rigorous analysis using better and more recent data is warranted.

2.2 Related Literature on Demographic Wage Gaps

The wage penalties associated with women, blacks, and Hispanics is well-established in the literature and there are decades worth of papers looking at the levels and trends of these wage gaps as well as reasons for why they might exist (Antonji and Blank, 1999; Blau and Kahn, 2000) with much of analysis based on Becker's taste-based approach (Becker 1957).

However, the literature on the cyclicality of these wage gaps is much thinner. The most relevant study, Biddle and Hamermesh (2012), finds evidence that women, Hispanics, and bad-looking workers experience greater earnings disadvantages during cyclical downturns and after experiencing negative industry-specific demand shocks. Biddle and Hamermesh also introduce an equilibrium search model of how macroeconomic fluctuations could induce either an increase or a decrease in wage gaps between two groups of workers, one of which is preferred by some employers.

Related studies have also looked at the disproportionate effects of the so-called Great Recession (2007-2009) on certain segments of the population, though without looking at earnings gaps in particular. Hoynes, Miller, and Schaller (2012) finds particularly adverse effects from the Great Recession on men, blacks, Hispanics, young, and less educated individuals. Elsby, Hobjin, and Sahin (2010) that the "young, male, less-educated, workers from ethnic minorities" were hit harder by this most recent downturn. And, lastly, a recent Pew study find that the Great Recession had much larger negative effects on young individuals without a college education than those with at least a college education (The Pew Charitable Trusts, 2013).

2.3 Other Related Literature on Business Cycles Effects

Dustmann, Glitz, and Vogel (2010) study the differential effects of business cycles on immigrants and natives in the UK and Germany. Consistent with the aforementioned studies on the effects of the Great Recession, the authors find "significantly larger unemployment responses to economic shocks for low-skill workers relative to high-skilled workers and for immigrants relative to natives within the same skill group."

Another strand of literature that is related to this study is the literature on the health effects of economic downturns. Indeed, as discussed earlier, disability and health are not time invariant. In a series of papers, Ruhm (2000, 2003, 2005, 2007, 2008) has found that, though conventional wisdom provides the opposite intuition, poor macroeconomic conditions actually improve physical health, including risk of coronary heart disease. This is despite an increase in income and use of medical services when the economy is better. Mental health, however, is worsens during economic downturns. Evidence points to a decline in physical activity and a decrease healthy behavior as working hours increase during economic expansions as a primary driver of this effect. Indeed, both smoking among heavy users and body weight among the obese drop during downturns. These effects matter for this study because the size of the disability earnings gap depends crucially on the characteristics of the population of people defined as disabled and health is another avenue through which economic conditions may affect disability status.

Macroeconomic conditions at time of graduation have also been shown to have persistent and negative effects on the labor market outcomes and health years down the road. Oreopoulos, Wachter and Heisz (2006) and Kahn (2010) show that those who graduate in a recession suffer earnings losses that do not disappear until 10 to 15 years after graduation. Maclean (2012), in a similar, study finds similar effects on health rather than labor market outcomes for those who graduate in a bad economy.

2.4 Measuring Disability

As previously discussed, the nature of disability is such that a "right" way of defining and measuring the disabled population is ambiguous at best. While the average person may characterize a paraplegic to be obviously disabled, someone with "milder" form of a spectrum disorder will be much more difficult to label as either "disabled" or "non-disabled". Also, depending on the job and accommodations made by the employer, that paraplegic may not even report himself as having a "work limiting" disability in a survey. The definition matters greatly, however, as shown in Autor and Duggan (2003), which finds that the most significant factor in the growth of disability insurance (SSDI) usage is the 1984 change in the SSDI screening rules that define disability for the program.

Even though there is a large literature spanning multiple fields concerned with various questions on disability and there are numerous laws, regulations, and social transfer programs targeted at the disabled, a clear and noncontroversial definition of disability does not exist. Mashaw and Reno (1996) document twenty definitions of disability that are actively used for determining eligibility for transfer programs, government services, and statistical analysis. Haveman and Wolfe (2000) show that the disability prevalence depends critically on the exact definition used, even within a specific dataset. With disability so difficult to nail down, definitions often differ based on the purpose of the determination or the study.

Complicating the matter for studies that use survey data where disability is self-reported is the fact that self-reporting of disability is influenced by social context (as with the paraplegic in the earlier example). Factors that may influence whether a person reports himself as being disabled or work limited include the availability of benefit/transfer programs and workplace conditions. If there are benefits to identifying oneself as being disabled, then over reporting is obviously a concern. For work limitation questions, someone with a physical, sensory, or mental limitation may not report himself as being disabled if the workplace provides sufficient accommodations for the disability or if the person's productivity at his job is not affected by the disability. Someone who is unemployed, on the other hand, may be more likely to report himself as being work limited compared to a person with the same condition or limitation who is employed. (Burkhauser, et al, 2010)

In addition to these concerns, self-reports of disability have also been shown to not be time invariant. This is true even when the underlying health of the person remains unchanged. (Kirchner, 1996) While the issues outlined above are obviously concerning, there is also some good news. Several studies have demonstrated that self-reports of work limitations do correlate well with more objective, medically determined measures of disability. (Bound and Burkhauser, 1999; LaRue, 1979; Maddox and Douglas, 1973; Nagi, 1969) In addition, Benitez-Silva et al (2004) compare self-reports of work-preventing disability in the Health and Retirement Study to the decisions made by medical professions on those individuals' applications for disability benefits from the Social Security Administration (SSA) and finds that they are "unable to reject the hypothesis that self-reported disability is an unbiased indicator of the SSA's decision." Lastly, Burkhauser et al (2001) compares the employment trends of the disabled population in the National Health Interview Survey (NHIS) and the CPS and finds them to be similar.

For this study, I have chosen to use the World Health Organization's International Classification of Functioning, Disability, and Health (the ICF model). In this model, disability is characterized by a dynamic relationship between a person's health, personal characteristics, and the physical and social environment in which he lives. These ICF concepts can be used to create a definition of disability. The first concept is that of a health condition, which, in the ICF model, is required to be present for a person to be considered disabled. The ICF model uses the International

Classification of Diseases, Tenth Edition (ICD-10) as a comprehensive listing of health conditions; included are diseases, injuries, health disorders, and other conditions (Wittenburg, et al, 2006; WHO, 2001).

The other concepts – impairment, activity limitation, and participation restriction – can be thought of as different categories of disability. Impairment is defined as "a significant deviation from, or loss in, body function or structure." In the SIPP Gold Standard, I am able to identify individuals with physical, mental, and sensory impairments. Examples include loss of limb, blindness, and the presence of a mental health condition such as ADHD or depression.

Activity limitation is defined as "difficulty in executing activities" and in my data, I can identify both those individuals who find it difficult to or cannot execute activities of daily living (ADLs) and individuals who find it difficult to or cannot execute instrumental activities of daily living (IADLs). Examples of ADLs are getting around the house, taking a shower, dressing, and eating. Examples of IADLS includes going outside the home and keeping track of money and bills.

Participation restriction is defined as the "inability to take part in conventional life situations for reasons that may be beyond his or her control." In the SIPP, the main participation restriction questions are whether a person is limited in his ability to work and whether a person has the ability to work at all. (Wittenburg, et al, 2006; WHO, 2001)

My analysis uses these six categories of disabilities based on these concepts from the ICF model. I look at individuals with work limitations, ADL limitations, IADL limitations, physical impairments, mental impairments, and sensory impairments. I define an individual as being disabled if he fits into at least one of these categories. My baseline results are computed for each of these six detailed categories of disability separately and also for two more aggregated categories – work limiting disabilities and difficulties, which includes ADL limitation, IADL limitation, physical impairments, mental impairments, and sensory impairments. The more aggregated categories take into account SIPP data on when the work limitation or difficulty began. The other specifications that are estimated in addition to the baseline results are only computed for work limiting disorders and difficulties. Table 1 details the specific SIPP questions that were used to construct each disability variable.

Indeed, one of the advantages of using the SIPP is that it allows for using this more comprehensive definition of disability rather than simply using a work limitation question, which many studies using other survey data are forced to do. The importance of being able to look beyond work limiting disabilities is underscored in Table 1.2, which shows cross tabulations of the various categories of disabilities. The table shows the percent of people who, conditional on being in the disability category shown in the rows, are also in the disability category in the column. In my data, while the incidence of any type of disability is much higher for those already in any of the other categories compared to the general population, it is also obvious that a much larger population is captured when all the disability categories are considered compared to when any single category, such as only those who describe themselves as having a work limiting disability, is considered.

3. DATA

For this study, I use a unique restricted access linked dataset. The SIPP Gold Standard File (GSF) is the result of a collaborative effort between the Census Bureau and the Social Security Administration (SSA). Staff at the Census Bureau have harmonized a subset of variables from nine panels (1984, 1990 - 1993, 1996, 2001, 2004, and 2008) of the Survey of Income and Program Participation (SIPP) and have linked the individual level records to administrative earnings and benefits records from the SSA.

The SIPP is series of short panels that span 2 to 4 years and sample approximately 12,000 to 50,000 households for each panel with the more recent ones having larger sample sizes. The first panel was in 1984 and the most recent is the 2008 panel. Each panel since the 1984 one has been integrated into the GSF. The structure of the SIPP is somewhat complex. Within a panel, households are interviewed once every four months and each interview is called a wave. Each panel of the SIPP since 1984 has consisted of 8 to 13 waves. During each wave, the month (out of the four) in which a household is interviewed is determined by its rotation group, which, in turn, determines the dates a household is referring to when answering questions. Each interview consists of the core questions, which are asked during each wave, and topical questions, which vary from wave to wave.

The SIPP Gold Standard consists of all households from the SIPP that have provided a valid Social Security Number, which allows them to be linked to the administrative records (match rate about 80-90 percent for all panels) [5]. Only a subset of the variables from the SIPP is in the Gold Standard. These include the following topic areas: demographics, marital history, labor market outcomes, welfare, health, and disability.

The annual earnings records come from employer-provided W-2 reports to the IRS, which are sent to the SSA. Depending on the level of detail, the earnings records span the years 1951 or 1979 to 2010. There are two types of earnings records – the Detailed Earnings Record (DER), which include non-top-coded deferred and non-FICA earnings dating back to 1978, and top-coded Summary Earnings Records (SER)

[5] Based on observed demographic characteristics, selection does not seem to be a problem. Previous work, such as Mazumder (2005) shows that correcting for selection using inverse probability weighting has little effect on regression results.

dating back to 1951. These earnings records all represent annual income, however, and unfortunately, there is no way to construct an hourly wage from these records. The Gold Standard also includes administrative records on benefit receipt from the SSA's Old Age, Survivor, and Disability Insurance (OASDI) and Supplemental Security Income (SSI) programs. These additional data allow me to explore how the self-reports of disability match up to the administrative records of disability insurance application and receipt, which are ultimately based medical evaluations and may be considered more objectives measures of disability.

The nature of the "panel" dataset I am using is somewhat complex. For each individual, any variables that come from the administrative side (annual earnings, SSA benefit receipt, etc.) are available for a much longer period of time than the variables that come from the SIPP. The SIPP variables are only available for the period of time that the person was in the SIPP for. While some of these are time-invariant (race, gender, date of birth, etc.), others, including disability, may change over time. Therefore, I am forced to make assumptions about these variables for the years in which the individual is not in the SIPP. I explain in more detail below where I have to make these assumptions. As an example, for someone in the 2004 SIPP panel, I have annual earnings data for that individual from 1978 to 2010, but I only observe the state of residence from 2004 to 2006 and I have to assume the state of residence from 1978 to 2003 and from 2007 to 2010.[6]

The annual earnings measure that I use in this study is a combination of the DER and the SER due to the difference in coverage between the two variables. Following the SIPP GSF Codebook and previous studies using these data[7] and after

[6] Robustness checks were done to test the sensitivity of these assumptions and in general, they did not qualitatively affect the main results. Also, as an additional robustness check, I estimated the baseline results while not using any observations from years after a person was in the SIPP and the results were qualitatively the same to those in Table 9.

[7] See, for example, Rutledge (2011).

examining the SER and DER records, I use the SER record for all years where the individual has zero non-FICA earnings and when the SER record is not top-coded. For the years where there are non-FICA earnings or where the SER record is top-coded at the FICA maximum, then I use the sum of the DER FICA earnings variable and the DER non-FICA earnings variable.[8]

There is also a total earnings variable and a total hours worked in the SIPP Gold Standard, which comes from the SIPP core questions. As previously described, these are only available for each individual for the years in which s/he is in the SIPP, which means that, for each individual, there are only a few years worth of these variables. I divide total earnings by total hours worked to obtain an hourly earnings variable for each individual. These only exist for when an individual is working.[9]

The disability variables that I use come from both the SIPP's core set of questions and Functional Limitation Topical Module, which is asked at least once for every panel between 1984 and 2008. Very detailed questions about medical conditions, physical, mental, and sensory limitations, as well as difficulty performing tasks are asked of all respondents at least 15 years old. These questions are used to create variables on the GSF that indicate whether each individual has work limitations, ADL limitations, IADL limitations, physical impairments, mental impairments, and/or

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[8] Results were also estimated using the sum of the DER earnings variables as a measure of annual earnings and just the SER as a measure of annual earnings. The directions of the effects of interest were the same using these annual earnings measures.

[9] When comparing results based on the annual earnings variables from the SSA data and the SIPP hourly earnings variable from the SIPP data, it is important to keep in mind the extensive literature showing where the administrative data and the survey data are sometimes at odds with each other. Gottschalk and Huynh (2010) assumes that the SSA data is the true measure of earnings and finds that workers with low earnings tend to overstate their earnings, while respondents with high earnings tend to underreport their earnings in SIPP. Cristia and Swasbish (2007) uses very similar methodology to find that measurement error in the survey data is negatively correlated with true earnings (administrative data). Dragoset and Fields (2008) uses the same data to find that results based on administrative and survey data are similar qualitatively but not identical that, quantitatively, magnitudes are often very different. Unlike the other studies, Abowd and Stinson (2011) do not assume that the administrative data is the correct measure of earnings. They find that the DER earnings variables are on average higher than the SIPP earnings variables and there is more variation due to unobservables in the administrative data than the survey data.

sensory impairments as defined by the ICF model of disability. Table 1.1 shows the original questions from the SIPP and which ICF disability category they were mapped to in creating the SIPP GSF.[10] The mapping is based on that used in Wittenburg and Nelson (2006). For each disability category, a person is defined as having that impairment/limitation is he answers yes to any of the questions in that category as defined in Table 1.1.[11]

The Functional Limitation topical module also asks when a person's work-limiting disability and difficulties (again, these are ADL or IADL restrictions and physical/mental/sensory impairments) began. This allows me to partially distinguish when a person is disabled from years when s/he is not. Unfortunately, each individual only answers the disability questions for the years where s/he is in the SIPP. Therefore, going back from the years the individual is in the SIPP, I can distinguish the years s/he was disabled from the years s/he was not, but going forward from those years to the end of the SSA data (2010), I have to assume that anyone who is disabled continues to be disabled.[12,13]

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[10] The later panels had more questions than the earlier panels and when the harmonization across panels was done, all available questions were used. In each panel, a person is defined as having a, for example physical impairment, if he answered yes to any of the physical impairment questions that were asked during that panel.

[11] The SIPP Gold Standard at this point looks at the answers to the work and functional limitations questions at a point in time and does not consider how the answers to questions that are asked more than once change. This might be cause for concern as it has been shown that self-reports sometimes change over time while underlying health conditions do not.

[12] For most detailed disability categories (ADL restriction, IADL restriction, physical impairment, mental impairment, and sensory impairment), I do not know when the disability began because many people are in multiple detailed disability categories and it is vague which is being referred to when the individual is asked the date the difficulty began. Since the results are estimated for both the more detailed disability categories and the more aggregated work limitation/difficulty disability categories and the same general qualitative results hold, these assumptions do not seem to matter too much. In addition, I estimated the baseline results assuming that year the difficulty began applies to each detailed disability category and the results were qualitatively the same to those in Table 1.10.

[13] As a robustness check, I estimated the baseline results using on those years where individuals were in the SIPP and years before individuals were in the SIPP and the results were very similar to those presented in Table 1.9.

TABLE 1.1: SIPP VARIABLES USED TO CREATE DISABILITY VARIABLES BY PANEL

	ICF Category	1996 - 2008 Panels	1990 - 1993 Panels	1984 Panel
Had a work-limiting physical or mental condition	Work Limitation	EDISABL	DISAB	SC1460
Had work-preventing physical/mental/health condition	Work Limitation	EDISPREV		
Health or condition preventing work	Work Limitation	EJOBCANT	TM8924, TM8332	TM8470
Use of a hearing aid	Sensory	EHEARAID		
Difficulty seeing words/letters in newspaper print	Sensory	ESEEDIF	TM8810	TM8336
Ability to see words and letters in print at all	Sensory	ESEENOT	TM8812	TM8338
Difficulty hearing what is said in conversation	Sensory	EHEARDIF	TM8814	TM8344
Ability to hear what is said at all	Sensory	EHEARNOT	TM8816	TM8346
Difficulty having speech understood	Sensory	ESPEECHD	TM8818	
Ability to understand speech at all	Sensory	ESPEECHC	TM8820	
Difficulty lifting and carrying 10 pounds	Physical	EDIF10	TM8822	TM8350
Ability to lift and carry 10 pounds at all	Physical	ECANT10	TM8824	TM8352
Difficulty lifting and carrying 25 pounds	Physical	EDIF25		
Ability to lift and carry a 25 pound bag at all	Physical	ECANT25		
Difficulty pushing or pulling large objects	Physical	EPUSHD		
Ability to push or pull large objects at all	Physical	EPUSHC		
Difficulty standing on being on feet	Physical	ESTANDD		
Difficulty sitting	Physical	ESITD		
Difficulty stopping, crouching, or kneeling	Physical	ESTOOPD		
Difficulty reaching over head	Physical	EREACHD		
Difficulty using hand and fingers	Physical	EGRASPD		
Ability to use hands and fingers at all	Physical	EGRASPC		
Difficulty walking up a flight of stairs	Physical	ESTAIRSD	TM8826	TM8358
Ability to walk up a flight of stairs at all	Physical	ESTAIRSC	TM8828	TM8360
Difficulty walking a quarter of a mile	Physical	EWALKD	TM8830	TM8354
Ability to walk a quarter of a mile at all	Physical	EWALKC	TM8832	TM8356
Difficulty using an ordinary telephone	Physical	ETELED	TM8834	
Ability to use a telephone at all	Physical	ETELEC	TM8836	
Learning disability	Mental	ELDIS	TM8902	TM8462**
Mental retardation	Mental	EMR	TM8904	
Developmental disability	Mental	EDEVDIS	TM8906	
Alzheimer's disease	Mental	EALZ	TM8908	

Table 1.1 Continued

	ICF Category	1996 - 2008 Panels	1990 - 1993 Panels	1984 Panel
Other mental or emotional condition	Mental	EOTHERM	TM8910	
Frequently depressed or anxious	Mental	EANXIOUS		
Trouble getting along with other people	Mental	ESOCIAL		
Trouble concentrating	Mental	ECTRATE		
Trouble coping with stresses	Mental	ECOPE		
Difficulty getting around inside of the home	ADL	EINDIF	TM8838	TM8366
Difficulty getting in and out of bed or a chair	ADL	EBEDDIF	TM8842	TM8370
Difficulty taking a bath or shower	ADL	EBATHDIF	TM8844	TM8424
Difficulty dressing	ADL	EDRESSD	TM8846	TM8424
Difficulty walking	ADL	EWALK2D	TM8848 *	
Difficulty eating	ADL	EEATDIF	TM8850	TM8424
Difficulty using or getting to the toilet	ADL	ETOILETD	TM8852	TM8424
Difficulty going outside the home	IADL	EOUTDIF	TM8840	TM8362
Difficulty keeping track of money or bills	IADL	EMONEYD	TM8854	
Difficulty preparing meals	IADL	EMEALSD	TM8856	TM8398
Difficulty doing light housework	IADL	EHWORKD	TM8858	TM8396
Difficulty taking the right amount of medicine	IADL	EMEDD		
Year when work limitation began		TLMTYR	TM8310	TM8456
Year when difficulty began		TYEAR1		

Note: * Not in 1990 Panel. **This question asks what condition causes the respondent's difficulties. If one of the mental conditions is specified as main condition causing difficulty, then the respondent is coded as having a mental disability in the SIPP GSF.

TABLE 1.2: DISABILITY CATEGORY CROSS TABULATIONS

Disability Category	Work Limitation	Difficulty	Activities of Daily Living Restriction	Instrumental Activities of Daily Living Restriction	Physical Impairment	Mental Impairment	Sensory Impairment
Work Limitation		57.76%	16.15%	18.98%	44.68%	24.22%	25.46%
Difficulty	6.55%		13.09%	12.18%	49.05%	36.98%	35.63%
Activities of Daily Living Restriction	6.71%	47.93%		62.86%	93.05%	35.35%	35.73%
Instrumental Activities of Daily Living Restriction	8.10%	45.82%	64.58%		89.87%	44.41%	37.93%
Physical Impairment	4.02%	38.91%	20.16%	18.95%		21.99%	21.58%
Mental Impairment	4.42%	60.05%	16.68%	20.23%	46.81%		20.32%
Sensory Impairment	4.90%	60.39%	16.54%	17.09%	46.11%	21.10%	

Note: This table reports the percent of observations (persons, not person-years) that are in column category conditional on being in row category

Table 1.3 checks how well the SIPP disability data matches with the SSA administrative data on disability insurance receipt. Those who receive disability insurance are required to have a doctor's examination to establish their inability to work. In my sample as a whole, just over 3 percent of the observations are on disability insurance. Of those who indicate in the SIPP that they are unable to work, just under half are on disability insurance. Of those with work limiting disabilities, but not work preventing disabilities according to the SIPP data, about 13 percent are on disability insurance and of those with difficulties that are not described as work preventing in the SIPP data, just over 6 percent are on disability insurance. These numbers show that, consistent with the previous literature on this issue, while the survey and the administrative data on disability are certainly very correlated, there remain significant differences in the disabled populations defined in each dataset.

TABLE 1.3: SSA DISABILITY VERSUS SIPP DISABILITY

SIPP Disability Category		No Work	Percentage on Disability Insurance: Yes	Percentage on Disability Insurance: No
Work Limitation	Yes	Yes	44.05%	55.95%
		No	13.44%	86.56%
	No	Yes*	11.74%	88.26%
		No	2.77%	97.23%
Difficulty	Yes	Yes	46.50%	53.50%
		No	6.24%	93.76%
	No	Yes**	28.13%	71.87%
		No	2.01%	97.99%
No Work	Yes	N/A	42.52%	57.48%
	No		1.73%	98.27%
Overall			3.12%	96.88%
Avg. Annual Earnings			$2,610	$30,304

Note: This table reports the percent of observations (person-years) that are receiving disability insurance payments according to the Social Security Administration's Master Beneficiary Records conditional on whether he/she has a work limiting disability, a difficulty, and/or a work preventing disability in the SIPP data. The overall percent of the sample that is receiving disability insurance payments is also reported as is the average annual earnings (according to the Social Security Administration's records) of those receiving and those not receiving disability insurance payments. *Only 0.16% of all observations have Work Limit = No, No work = yes; ** Only 0.74% of all observations have Difficulty = No, No work = yes.

Also, the proportion of the observations that self-report as being disabled is not constant over the business cycle. As shown in Chart 1.1, both the proportion that report work limiting disabilities and the proportion that report difficulties seem to increase with the level of the state unemployment rate. These results would be consistent with one might expect based on intuition and the previous literature on the time-variant nature of self-reports on disability status.

CHART 1.1: PROPORTION DISABLED BY UNEMPLOYMENT RATE

I merged the GSF with state level unemployment rates for each year between 1978 and 2010 from the Bureau of Labor Statistics. Chart 1.2 shows the unemployment rate in each state for each year between 1978 and 2012 as a point as well as the national average (black line). It is clear from this chart that, each year, the range between the state with the lowest unemployment rate and the state with the highest unemployment rate is large. Even in a year with little variance, such as 2000, the state with the lowest level of the unemployment rate was at 2.3% while the state with the highest level was at 6.7%. In a year with a very high level of variance, such as 1983, state unemployment rates ranged from 5.3% to 17.4%.[14]

[14] As an extension of the analysis presented in the Results section, I also estimated the baseline results using the state level employment to population ratio rather than the unemployment rate. The results were very similar to the results in Table 9. This should not be very surprising as, in general, the state level employment to population ratio tracks the state level unemployment rate pretty well.

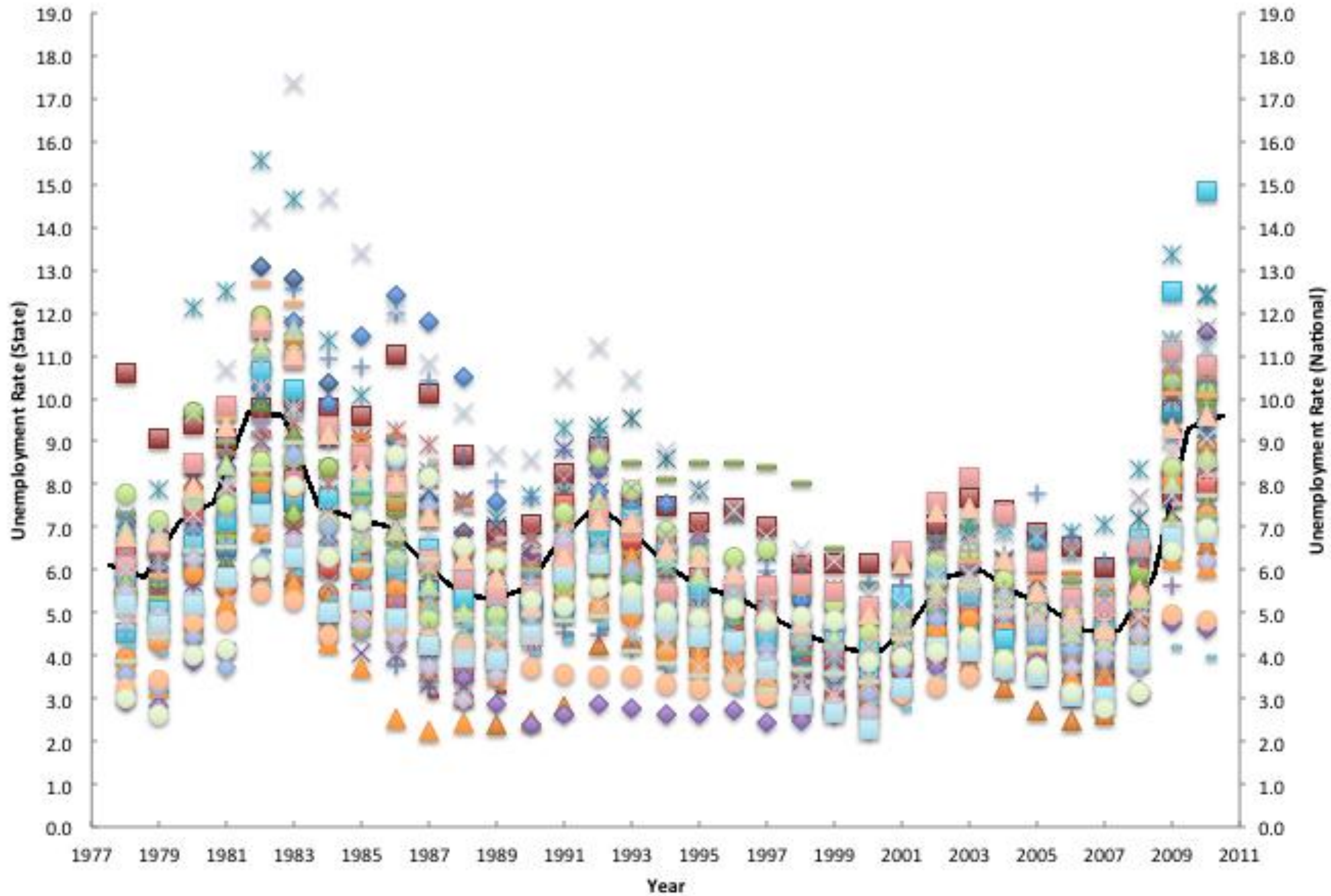

CHART 1.2: STATE UNEMPLOYMENT RATES BY YEAR: 1978-2010

To create my analysis sample, I restrict the person-year observations to only those where the individual was between the ages of 25 and 64 to reduce any confounding issues with education and retirement. For people who indicate in the SIPP that their disability prevents them from working, I use that information along with when they started not being able to work to eliminate those person-year observations since I am interested in looking at comparing those who are disabled but still able to work to those who are not disabled.

I also eliminate those who are out of scope for disability questions, those with missing values for disability questions, those without a valid Social Security number (for matching to the administrative data), those whose disability prevents them with working, those with missing earnings data, those with missing values for the control variables, and those with annual earnings of zero every year between 1978 and 2010. The reductions to the sample size caused by each of these cuts to the data are show in Table 1.4. The greatest reduction by far comes from eliminating those person-years under age 25 and those over age 64. This is especially true for the SSA Sample as the earlier years are skewed toward older individuals due to the structure of the data.

For the analysis where I am looking at annual earnings, my total sample size is 5,752,980 person-years. These represent about 255,398 people, which means that each person is represented with 22.5 years of data on average. The samples sizes for the analysis where I am looking at hourly earnings are much smaller at about 380,323 person-years, representing just over 168,816 people, which means each person is represented with just over 2 years of data on average. The disparity reflects the structure of the linked survey-administrative dataset. There are fewer years associated with each person in the analysis of hourly earnings because these only use the years where each individual was in the SIPP rather than each person's SSA records from 1978 to 2010.

TABLE 1.4: SAMPLE SIZES

CUT TO SAMPLE	Person-Years	
	Person-Years	Persons
	25,628,229	776,613
Age 25 to 64	9,284,804	460,130
Disability not in scope and SSN not valid	8,876,017	407,254
Work Preventing Disability	8,603,849	402,485
Disability information missing	6,523,088	292,808
Earnings information missing	6,431,787	287,794
Control variables missing	5,778,002	257,929
No Years of Positive Earnings	5,752,980	255,398

TABLE 1.5: SIPP PANELS

	SSA Sample		SIPP Sample		Number of Wave 1 Eligible Households
SIPP PANEL	Person-Years	Persons	Person-Years	Persons	
1984	377,027	15,411	17,909	9,693	20,897
1990	693,230	28,642	34,739	18,617	19,800
1991	425,611	17,622	21,575	11,526	15,626
1992	553,926	23,117	28,746	15,213	21,577
1993	581,821	24,537	29,790	16,001	21,823
1996	888,141	40,114	91,409	26,860	40,188
2001	515,756	24,911	30,123	15,908	50,500
2004	906,362	42,646	78,179	29,168	51,379
2008	811,106	38,398	47,853	25,830	52,030

Note: This table shows the number of person-years and the number of persons in each of the analysis samples that came from each of the nine SIPP panels as well as the number of households initially eligible to be interviewed for that SIPP panel.

Table 1.5 shows how many persons and person-years are from each of the ten panels of the SIPP that are in the GSF. In addition, I list the number of households initially eligible for interview in wave 1 of the panel.[15] The sample size from each panel roughly corresponds with the size of each panel, as one would expect.

Table 1.6 contains the summary statistics for the sample where I am looking at annual earnings ("the SSA sample"). The first column represents the entire sample, while the second column represents the person-years where the person has zero annual earnings. The next few sets of columns present the disability and demographic comparisons. For the SSA sample, these summary statistics show that those without disabilities are generally younger, better educated, have more work experience, more likely to be married, and more likely to be in managerial or professional careers. These trends are consistent with other research (Haveman and Wolfe, 2000). Also, women, blacks, and Hispanics are less educated, less likely to be married, more likely to have children, and are in very different industries when compared to their counterparts. Lastly, while race and ethnicity comparisons do not show large differences across industries, women are in very different industries than men.

Table 1.7 shows the same comparisons for the sample where I am looking at hourly earnings ("the SIPP sample"). While many of the same broad trends generally seem to be true in this sample, there are differences since this sample is much smaller than the SSA sample. For example, in the SSA sample, disabled individuals have less work experience, but in the SIPP sample, disabled individuals have more work experience.

[15] The number of wave 1 eligible households is taken from the SIPP User's Guide, Chapter 2. URL: http://www.census.gov/sipp/usrguide/ch2_nov20.pdf

TABLE 1.6: SUMMARY STATISTICS (ANNUAL EARNINGS SAMPLE)

		ALL	Zeros	No Work Limit	Work Limit	No Difficulty	Difficulty	Male	Female	Not Black	Black	Not Hispanic	Hispanic
N (Person-Years)		5,752,980	894,476	5,647,567	105,413	5,127,922	625,058	2,738,299	3,014,681	5,163,266	589,714	5,301,028	451,952
Persons		255,398	134,253	252,853	2,545	232,966	22,432	121,754	133,644	227,946	27,452	233,891	21,507
Age (years)		42	44	42	46	41	45	42	42	42	41	42	40
Annual Earnings (Year 2000 $)		$30,511	$0	$30,745	$17,955	$31,384	$23,347	$41,723	$20,327	$31,469	$22,120	$31,367	$20,473
Disabled (Work Limit) %		1.8	3.4			0.9	9.9	2.0	1.6	1.9	1.7	1.9	1.3
Disabled (Difficulty) %		10.9	14.4	10.0	58.6			10.0	11.7	10.8	11.3	10.9	10.2
Female %		52.4	69.5	52.5	47.0	51.9	56.4			51.6	59.9	52.4	52.9
Black %		10.3	10.9	10.3	9.2	10.2	10.6	8.6	11.7			10.5	7.7
Hispanic %		7.9	10.6	7.9	5.8	7.9	7.4	7.8	7.9	8.1	5.9		
Married %		66.4	67.9	66.6	57.8	67.2	60.0	70.2	63.1	69.0	44.3	66.7	63.7
Have Children %		75.0	81.1	75.0	72.6	74.6	78.4	72.1	77.7	74.7	77.9	74.6	79.9
Education (less than HS) %		12.2	19.5	12.1	20.5	11.5	18.3	12.8	11.7	11.5	18.7	10.1	37.0
Education (HS Degree) %		30.7	34.4	30.6	36.8	30.5	33.1	29.3	32.0	30.4	34.1	31.0	28.2
Education (Some College) %		30.4	26.5	30.4	29.7	30.5	30.1	29.3	31.4	30.3	31.8	31.0	24.0
Education (College Degree) %		17.1	13.6	17.3	8.9	17.8	12.0	17.8	16.5	17.9	10.3	18.0	7.4
Education (Graduate Degree) %		9.5	6.0	9.6	4.2	9.9	6.5	10.8	8.3	10.0	5.1	10.0	3.4
Experience (years)		22.0	24.5	21.9	27.4	21.5	25.9	22.0	22.0	22.0	22.0	22.0	21.8
Industry	Manufacturing %	14.0	8.6	14.0	15.1	14.1	13.4	19.2	9.3	14.1	13.3	14.0	15.1
	Wholesale/Retail Trade %	13.7	11.7	13.7	16.0	13.9	12.5	13.9	13.5	14.0	11.1	13.6	14.5
	Fire, Services, Public Admin, Military %	41.6	31.5	41.7	35.0	42.0	37.7	31.3	50.9	40.9	47.6	41.9	37.7
	Ag, Mining, Const., Trans., Comm., Pub. Util.%	11.6	8.1	11.6	10.3	11.7	10.3	18.7	5.1	11.7	10.5	11.4	14.2
Occupation	Managerial and Prof Specialty %	25.3	13.7	25.4	14.9	26.1	18.7	25.5	25.0	26.2	17.1	26.3	13.2
	Technical, Sales, and Administrative Support %	24.3	18.9	24.3	23.6	24.6	21.7	15.1	32.7	24.4	23.4	24.7	20.2
	Other %	31.2	27.1	31.1	37.9	31.0	33.4	42.5	21.0	30.0	41.9	29.8	48.0

TABLE 1.7: SUMMARY STATISTICS (HOURLY EARNINGS SAMPLE)

		ALL	No Work Limit	Work Limit	No Difficulty	Difficulty	Male	Female	Not Black	Black	Not Hispanic	Hispanic
	N (Person-Years)	380,323	374,267	6,056	333,414	46,909	188,137	192,186	340,597	39,726	348,925	31,398
	Persons	168,816	165,460	3,356	148,627	20,189	82,757	86,059	151,057	17,759	154,721	14,095
	Age (years)	42	42	44	41	45	42	42	42	41	42	40
	Hourly Earnings (Year 2000 $)	$19.52	$19.63	$12.78	$19.78	$17.61	$23.37	$15.75	$20.07	$14.78	$20.02	$13.92
	Disabled (Work Limit) %	1.6			0.8	7.4	1.7	1.5	1.6	1.2	1.6	1.1
	Disabled (Difficulty) %	12.3	11.6	57.5			10.5	14.2	12.4	12.0	12.4	11.2
	Female %	50.5	50.6	46.7	49.5	58.1			49.4	59.9	50.7	48.2
	Black %	10.5	10.5	7.8	10.5	10.1	8.5	12.4			10.8	6.5
	Hispanic %	8.3	8.3	5.8	8.4	7.5	8.7	7.9	8.6	5.1		
	Married %	67.3	67.4	61.6	68.3	60.3	72.2	62.5	69.7	46.9	67.4	66.2
	Have Children %	74.0	73.9	75.5	73.5	77.5	71.6	76.3	73.3	79.3	73.4	80.7
	Education (less than HS) %	8.8	8.6	19.2	8.2	12.8	9.8	7.8	8.4	12.5	6.8	31.2
	Education (HS Degree) %	29.5	29.3	38.4	29.1	31.9	29.1	29.9	29.0	33.5	29.5	28.6
	Education (Some College) %	31.9	31.9	29.2	31.6	33.8	30.5	33.2	31.5	35.6	32.3	27.6
	Education (College Degree) %	19.7	19.8	8.8	20.5	13.9	19.8	19.5	20.5	12.8	20.6	9.0
	Education (Graduate Degree) %	10.2	10.3	4.4	10.6	7.5	10.8	9.6	10.7	5.5	10.8	3.7
	Experience (years)	21.8	21.7	25.3	21.2	25.8	21.8	21.8	21.8	22.1	21.9	21.2
Industry	Manufacturing %	17.6	17.6	20.5	17.6	18.0	23.5	11.9	17.9	15.8	17.6	18.3
	Wholesale/Retail Trade %	15.2	15.2	19.0	15.2	15.4	15.5	15.0	15.6	11.8	15.1	16.3
	Fire, Services, Public Admin, Military %	52.6	52.7	46.1	52.5	53.2	38.4	66.4	51.7	59.6	53.1	47.0
	Ag, Mining, Const.,Trans., Comm., Pub. Util.%	14.3	14.3	13.7	14.5	13.0	22.4	6.3	14.5	12.6	14.0	18.0
Occupation	Managerial and Prof Specialty %	33.4	33.6	19.9	34.2	27.7	32.5	34.3	34.7	22.5	34.8	17.9
	Technical, Sales, and Administrative Support %	29.4	29.3	30.9	29.3	29.6	17.9	40.6	29.4	29.1	29.8	24.1
	Other %	36.8	36.7	48.5	36.1	42.1	49.3	24.6	35.5	48.1	35.0	57.5

4. METHODS

I start by looking at a simple breakdown of average annual earnings (SER and DER) for each level of the unemployment rate by disability status for each category of disability and by gender, race, and ethnicity. These are shown in Charts 1.3 through 1.7. The secondary axes on these graphs show the percent of the total sample of person-years at that unemployment rate. Charts 1.8 through 1.12 are similar, but show the percent of observations that have non-zero earnings (an "employment rate") by disability status and gender, race, and ethnicity at each rounded unemployment rate.

In addition to these simple averages, I also use regression analysis utilizing both the administrative annual earnings records (SER and DER) and the hourly earnings from the SIPP.

The general equation I am estimating for the disability comparisons is:

$$I_{ist} = \beta_0 + \beta_1 D_{ist} + \beta_2 U_{st} + \beta_3 U_{st} D_{ist} + \beta_4 X_{ist} + v_s + \tau_t + \varepsilon_{st} \qquad (1)$$

I_{ist} is the natural log of individual *i*'s annual earnings in a given year *t* and *s* denotes the state that individual *i* lives in. D_{ist} is an indicator variable for whether individual *i* is disabled. As discussed before, disabled here can mean having work limitations or a difficulty, which includes ADL limitations, IADL limitations, physical impairments, mental impairments, and/or sensory impairments. Separate results are estimated for both work limiting disabilities and difficulties. The baseline analyses are also estimated for each detailed type of disability – work limitation, ADL limitations, IADL limitations, physical impairments, mental impairments, and/or sensory impairments.

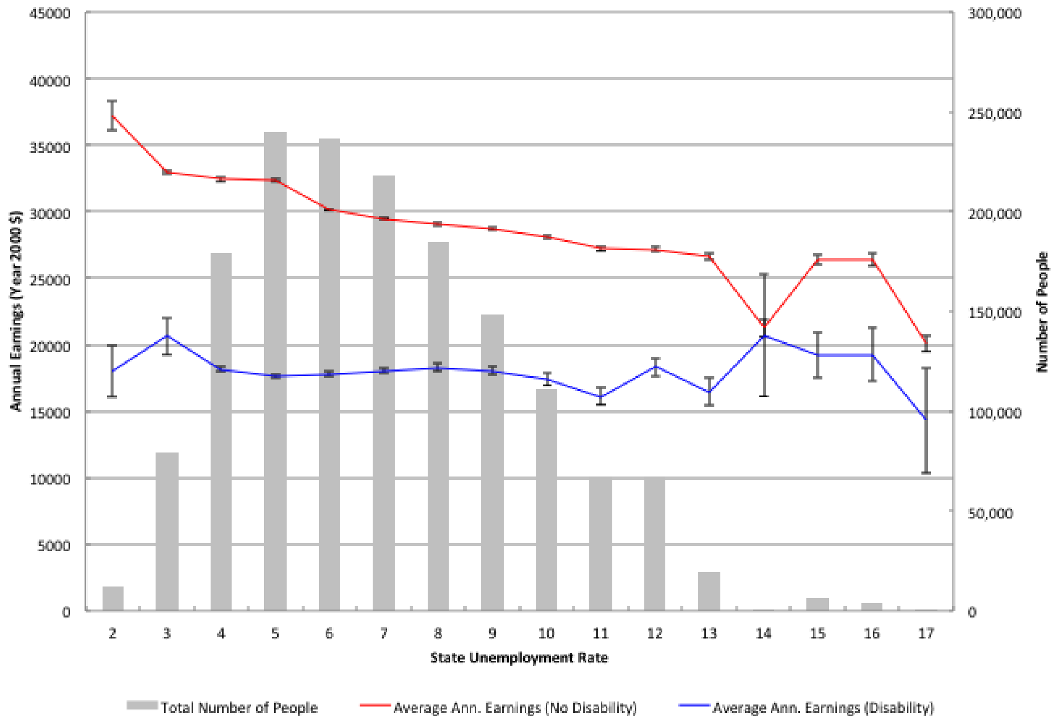

CHART 1.3: AVERAGE ANNUAL EARNINGS BY UNEMPLOYMENT RATE LEVEL - DISABILITY (WORK LIMITATION)

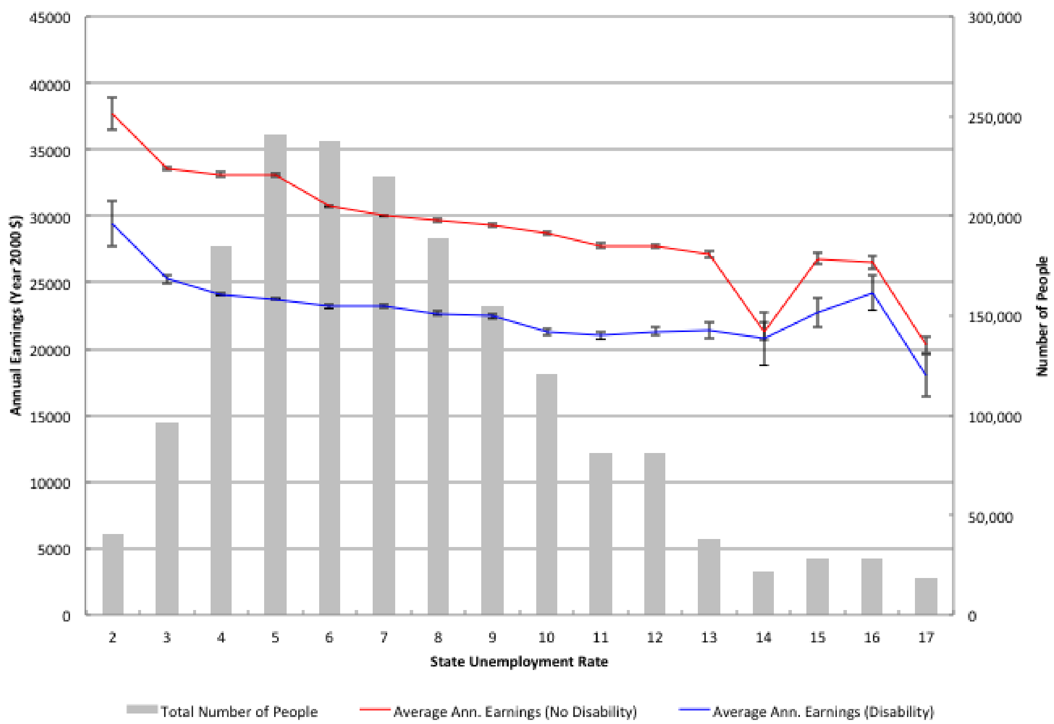

CHART 1.4: AVERAGE ANNUAL EARNINGS BY UNEMPLOYMENT RATE LEVEL - DISABILITY (DIFFICULTY)

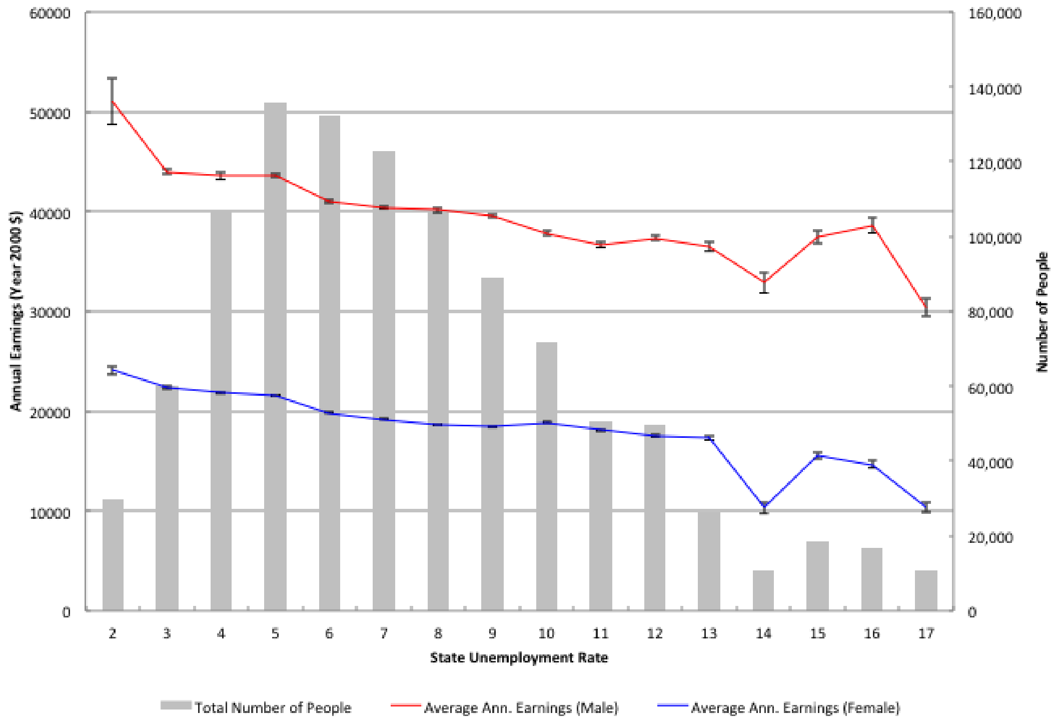

CHART 1.5: AVERAGE ANNUAL EARNINGS BY UNEMPLOYMENT RATE LEVEL – GENDER

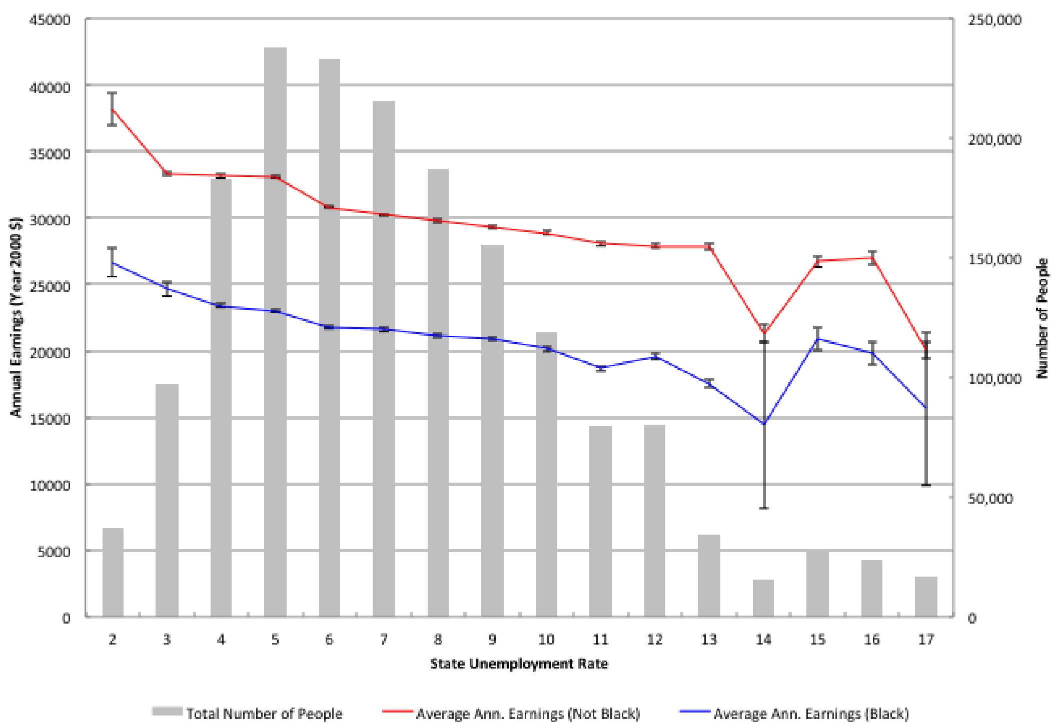

CHART 1.6: AVERAGE ANNUAL EARNINGS BY UNEMPLOYMENT RATE LEVEL - RACE

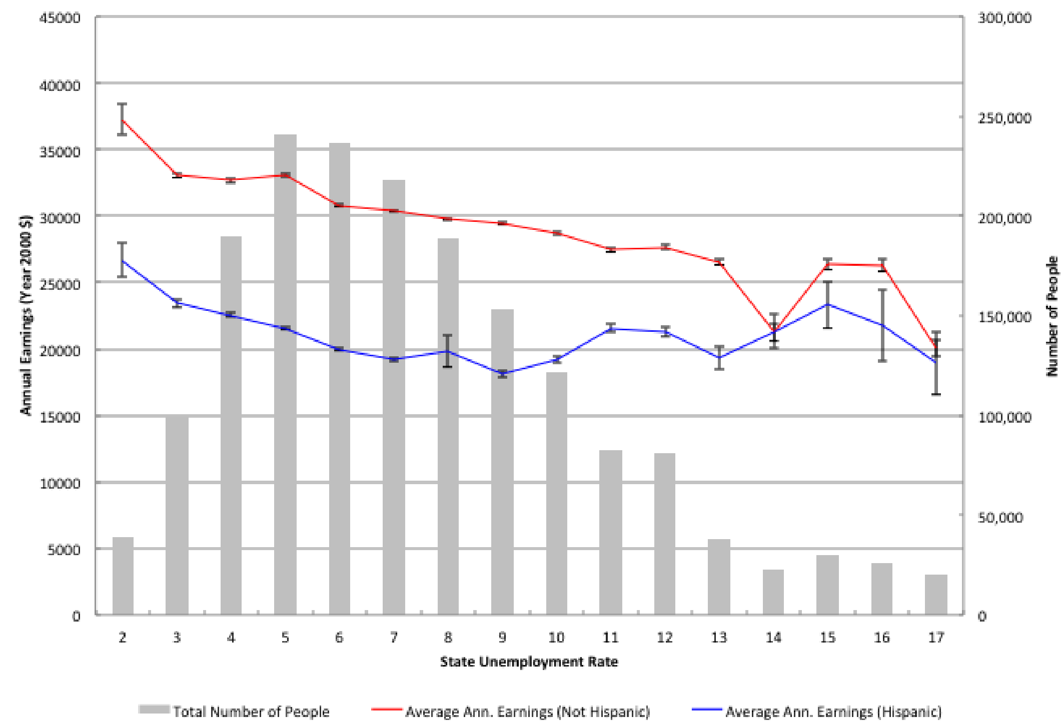

CHART 1.7: AVERAGE ANNUAL EARNINGS BY UNEMPLOYMENT RATE LEVEL - ETHNICITY

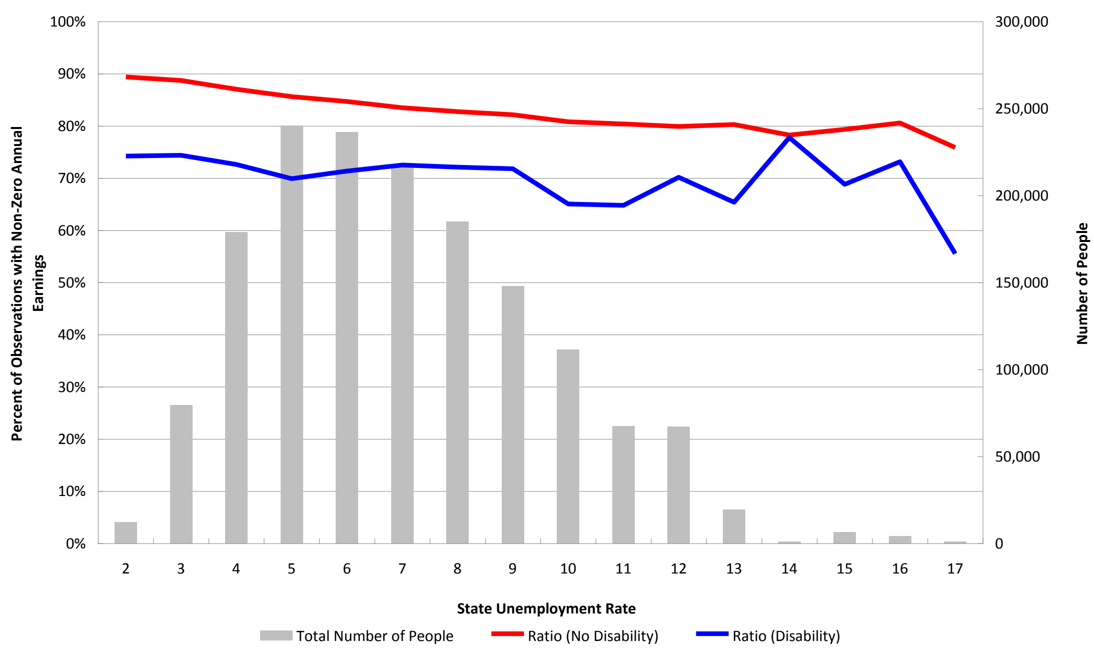

CHART 1.8: EMPLOYMENT RATE BY UNEMPLOYMENT RATE LEVEL - DISABILITY (WORK LIMITATION)

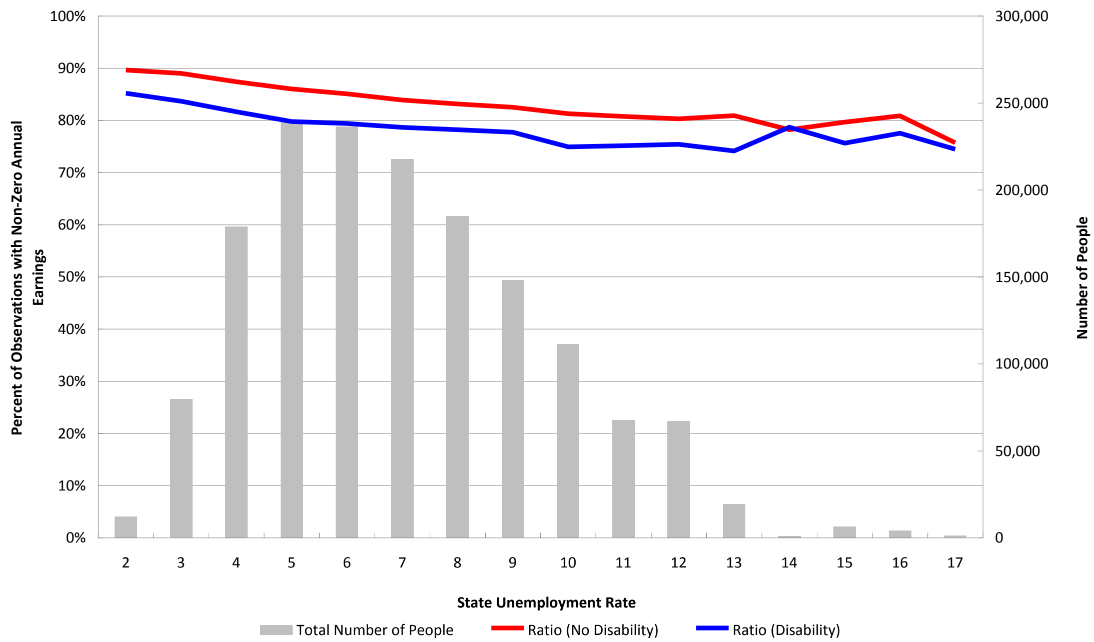

CHART 1.9: EMPLOYMENT RATE BY UNEMPLOYMENT RATE LEVEL - DISABILITY (DIFFICULTY)

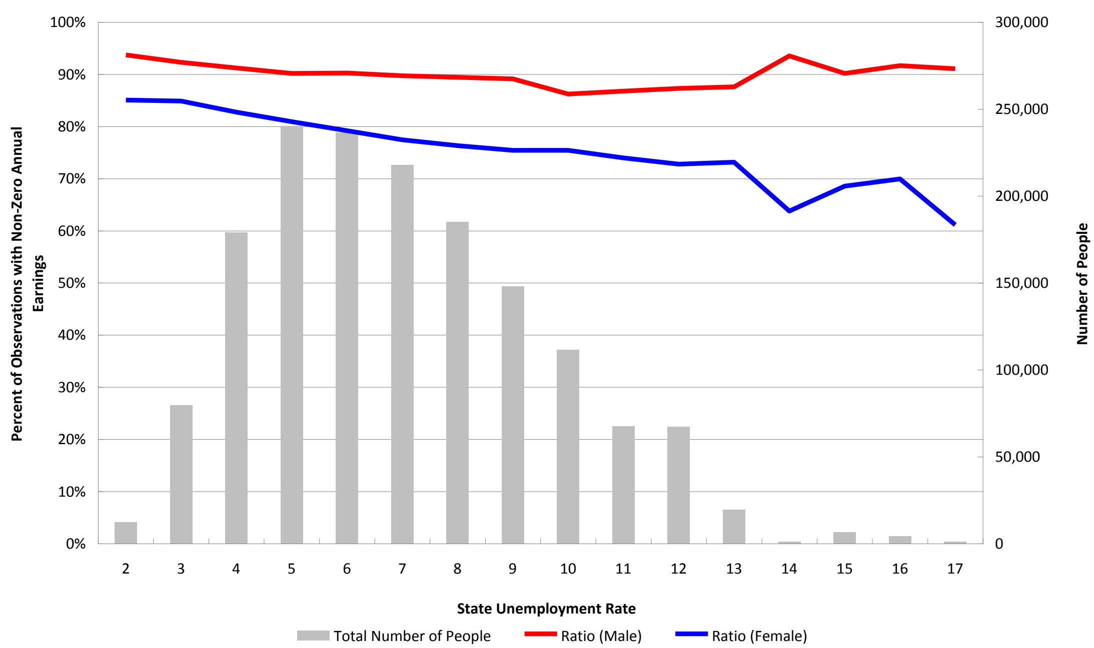

CHART 1.10: EMPLOYMENT RATE BY UNEMPLOYMENT RATE LEVEL - GENDER

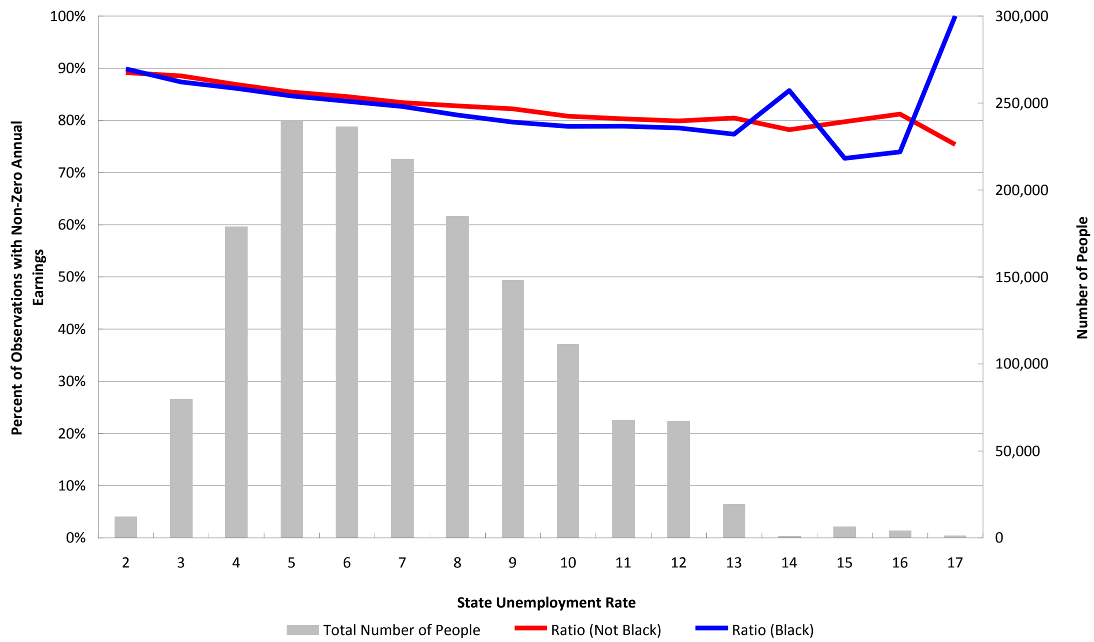

CHART 1.11: EMPLOYMENT RATE BY UNEMPLOYMENT RATE LEVEL - RACE

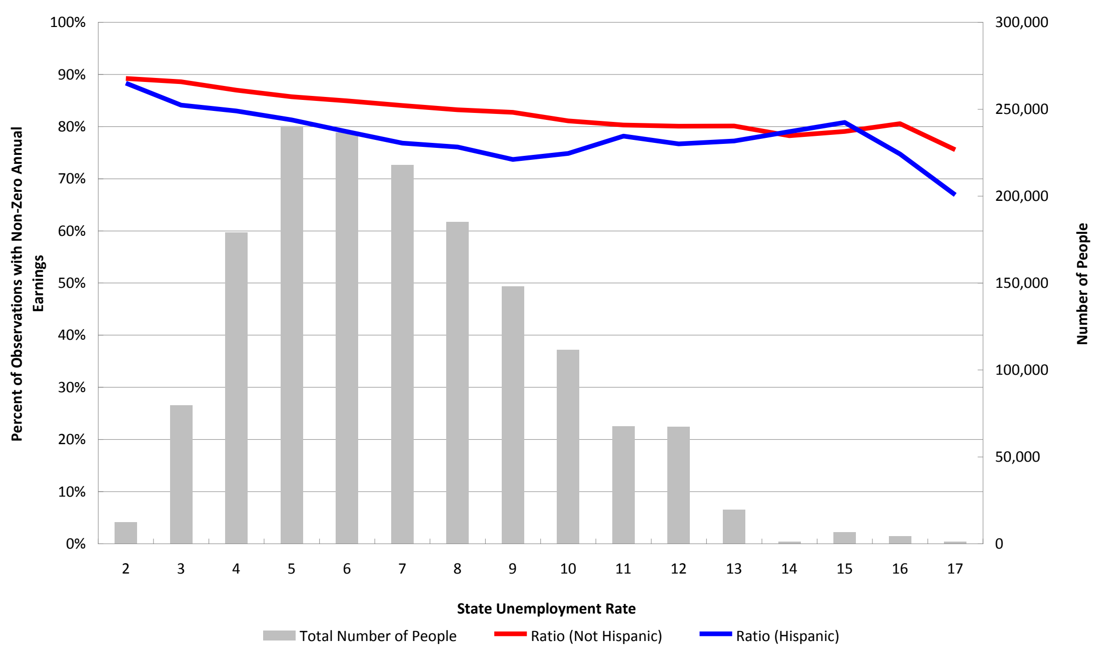

CHART 1.12: EMPLOYMENT RATE BY UNEMPLOYMENT RATE LEVEL - ETHNICITY

U_{st} is the unemployment rate in state s at time t. X_{ist} is a vector of control variables that include gender, race, ethnicity, educational attainment, marital status, number of children, experience, occupation and industry. One reason to control for occupation and industry is that the labor market effects of disability could vary by industry and occupation. For example, Kruger and Kruse (1995) find that those with computer skills do not suffer as negative of an earnings effect after the onset of a disability.

v_s are state fixed effects and τ_t are year fixed effects.[16,17] The year fixed effects allow me to abstract away from long-term aggregate labor market trends among minorities, women, and the disabled as well as changing attitudes toward these groups. In addition, some states may experience higher or lower than average (national average) unemployment for long periods of time and these long-term state-specific trends could be correlated with wage gaps between demographic groups or the disability wage gap as well as attitudes toward certain groups. Including state fixed effects allows me to focus on temporary variation in state-specific labor market conditions.

For the demographic comparisons, the equation is very similar:

$$I_{ist} = \beta_0 + \beta_1 M_{ist} + \beta_2 U_{st} + \beta_3 U_{st} M_{ist} + \beta_4 X_{ist} + v_s + \tau_t + \varepsilon_{st} \qquad (2)$$

[16] These results do no make use of weights. Because of the nature of the SIPP Gold Standard File, Census Bureau staff generally do not recommend using the person level weights in the SIPP data and other researchers using these data also estimate unweighted regressions. As robustness checks, I also estimated the baseline results using various person level weights and the results do not qualitatively differ from those presented in Table 1.7.

[17] The baseline results were also estimated using leads and lags of the local unemployment rate. This did not change the results appreciatively.

The only difference is that M_{ist} is an indicator variable for whether individual i is female (for the gender gap analysis), black (for the race gap analysis), or Hispanic (for the ethnicity gap analysis).

When using administrative earnings records, I am able to look at the years 1978 to 2010. Even though the SIPP has a complex sequential and sometimes overlapping panel structure, there are SIPP earnings records for at least part of the year for all the years between 1984 and 2010. Therefore I also estimate equations (1) and (2) using hourly earnings variables from the SIPP.

Since there is a social insurance program for the disabled and the decline in labor force participation among the disabled is well documented in the literature, I am also interested in the disabled, women, blacks, or Hispanics responding to adverse labor market conditions by leaving or being "forced" to leave employment and I estimate separate regressions removing years of zero annual earnings and keeping years of zero annual earnings. When keeping the zeros, I only keep the ones that are between years of non-zero annual earnings.

Because of the structure of the data, I can only observe the state an individual lives during the time he is in the SIPP. I assume that for years before the first observation in the SIPP, the individual lives in the same state we first observe him in and for years after the last observation in the SIPP, he lives in the state we last observe him in. Similar to the assumptions made about state of residence, I also have to make the assumptions about the time invariance of other control variables that are only available from the SIPP for the analysis using administrative earnings records.

As variations of the baseline estimates using equations (1) and (2), I also estimated variations of these equations using, instead of the unemployment rate, an indicator for the unemployment rate being greater than 6% (using both annual and hourly earnings variables). This can be interpreted a looking at years where is the local labor market is “good” versus years where local labor market conditions are “bad.” Lastly, I also estimated equations (1) and (2) by business cycle; that is, looking at the following periods of time in isolation to see if the effects of interest change over time: 1980-1990, 1990-2001, and 2001-2007.

5. RESULTS

The simple averages of annual earnings at different levels of the state unemployment rate (rounded to the nearest integer) are shown in Charts 1.3 through 1.7. The disability comparisons (Charts 1.3 and 1.4) both seem to show that the gap between the non-disabled and the disabled is larger at lower levels of unemployment. Of course, it is important to keep in mind that the sample sizes are skewed toward lower unemployment rates with the higher levels of unemployment more scarcely populated and the average earnings measure also more noisy at those levels of unemployment. This is shown by the bars that represent the percent of the sample in terms of people (not person-years) at each level of the unemployment rate.

Chart 1.5 shows the gender comparisons and there does not appear to be a clear effect either way looking just at the average level of annual earnings. Chart 1.6 is the race comparison, which looks similar to the gender comparison in that a simple visual comparison does not yield a clear direction of effect. Chart 1.7, however, the ethnicity comparison, looks more similar to the disability comparisons in Charts 1.3 and 1.4 than the gender and race comparisons in Charts 1.5 and 1.6 in that the earnings gaps appears to be smaller at higher levels of the unemployment rate.

Charts 1.8 through 1.12 show the “employment rate” by disability status, gender, race, and ethnicity as well as the rounded level of the local unemployment rate. While the fraction of observations with positive annual earnings decreases more for women as the local unemployment rate increases than it does for men, we do not see this same patter for the disabled, blacks, and Hispanics. Again, the data is much noisier for these groups, especially at higher levels of unemployment where the sample sizes decrease drastically.

Before discussing the regression results, I first ran some logit regressions estimating the effect of disability, gender, race, and ethnicity on the probability of having zero annual earnings and the probability of having zero annual income from labor market earnings and disability insurance payments. As mentioned before, my baseline analysis is done on a sample that includes years of zero annual earnings and a sample that does not, so it is first instructive to look at the differences in likelihood of having zero annual earnings among these groups. These results are shown in Table 1.8. Each panel is separate group comparison and within each, the first column shows the results for when an indicator variable for zero annual earnings is the dependent variable and the second column shows the results for when an indicator variable for the sum of annual earnings and disability insurance payments is zero. The results show that the disabled, especially those with work limiting disabilities, women, and Hispanics are more likely to experience years of zero earnings. The inclusion of disability insurance payments partially reduces these effects for the two disability categories, but does not for women and Hispanics, as would be expected.

TABLE 1.8: LOGIT RESULTS - PROBABILITY OF ZERO EARNINGS/INCOME

DISABILITY (WORK LIMITIMATION)

	Annual Earnings	Annual Earnings + Disability Benefits
Odds Ratio	2.155	1.393
Estimate	0.768	0.332
Std. Error	0.013	0.009
Controls		
N (Person-Years)	5,752,980	5,752,980
Persons	255,398	255,398

DISABILITY (DIFFICULTIES)

	Annual Earnings	Annual Earnings + Disability Benefits
Odds Ratio	1.166	1.015
Estimate	0.154	0.015
Std. Error	0.006	0.005
Controls		
N (Person-Years)	5,752,980	5,752,980
Persons	255,398	255,398

GENDER

	Annual Earnings	Annual Earnings + Disability Benefits
Odds Ratio	2.449	2.629
Estimate	0.896	0.967
Std. Error	0.012	0.012
Controls		
N (Person-Years)	5,752,980	5,752,980
Persons	255,398	255,398

RACE

	Annual Earnings	Annual Earnings + Disability Benefits
Odds Ratio	0.983	0.923
Estimate	-0.018	-0.080
Std. Error	0.008	0.008
Controls		
N (Person-Years)	5,752,980	5,752,980
Persons	255,398	255,398

Table 1.8 Continued

	ETHNICITY	
	Annual Earnings	Annual Earnings + Disability Benefits
Odds Ratio	1.158	1.176
Estimate	0.147	0.162
Std. Error	0.021	0.021
Controls		
N (Person-Years)	5,752,980	5,752,980
Persons	255,398	255,398

Note: This table shows the odds ratio for the coefficient of interest (on the disability/gender/race/ethnicity variable) as well the associated estimate and standard error for each logit regression where an indicator variable for zero annual earnings/income is the dependent variable. Each column is a separate regression where the column heading is the dependent variable. Each panel is the relevant comparison (disability category, gender, race, or ethnicity). Controls include gender, race, ethnicity, marital status, whether s/he has children, education, experience in years, the square of experience in years, industry group, occupation group, state, and year.

TABLE 1.9: BASELINE REGRESSION RESULTS

DISABILITY (WORK LIMITATION)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR	0.065	0.008	0.016
Std. Error	0.014	0.003	0.006
T-Stat	4.71	3.15	2.62
Controls			
Avg. Group Disadvantage	-1.42	-0.40	-0.22
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

DISABILITY (DIFFICULTIES)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR	0.022	0.007	0.003
Std. Error	0.006	0.001	0.002
T-Stat	3.41	5.12	1.58
Controls			
Avg. Group Disadvantage	-0.32	-0.15	-0.08
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

GENDER

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Female * UR	-0.076	-0.007	0.004
Std. Error	0.013	0.003	0.002
T-Stat	-5.97	-2.44	2.15
Controls			
Avg. Group Disadvantage	-1.55	-0.63	-0.33
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

RACE

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Black * UR	-0.022	0.004	-0.003
Std. Error	0.004	0.002	0.002
T-Stat	-5.23	2.22	-1.56
Controls			
Avg. Group Disadvantage	-0.06	-0.10	-0.08
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

Table 1.9 Continued

	DISABILITY (WORK LIMITATION)			**DISABILITY (DIFFICULTIES)**		
	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings

	ETHNICITY		
	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Hispanic * UR	-0.037	-0.003	-0.003
Std. Error	0.024	0.002	0.002
T-Stat	-1.53	-1.45	-1.14
Controls			
Avg. Group Disadvantage	-0.30	-0.12	-0.14
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

Note: This table shows the coefficient of interest (on the disability/gender/race/ethnicity times unemployment rate variable) as well the associated standard error and t-stat for each regression. Each column is a separate regression where the column heading is the dependent variable. Each panel is the relevant comparison (disability category, gender, race, or ethnicity). Controls include gender, race, ethnicity, marital status, whether s/he has children, education, experience in years, the square of experience in years, industry group, occupation group, state, and year. Average group disadvantage is expressed in log points.

The results show that for both work limiting disabilities and difficulties, the coefficient on the disabled*unemployment rate term is positive and significant for the regressions using annual earnings both with and without years of zero earnings. This indicates that as the unemployment rate goes up, the gap between the disabled for each disability category and the non-disabled in terms of annual earnings decreases. For hourly earnings, the coefficient of interest remains positive and significant for work limiting disabilities and for difficulties, the coefficient remains positive, but loses significance. All together, the baseline results for disability point to the result that as the state unemployment rate increases, the gap in earnings between the disabled and non-disabled decreases.

For the demographic comparisons shown in Table 1.7, for annual earnings with zeros included, the gender gap seems to increase with the unemployment rate as does the race gap, but the ethnicity gap is insignificant though the point estimate is negative. For both the gender and ethnicity gap, the results for annual earnings without zeros are the same as those with zeros, but, interestingly, for the black-white gap, including years of zero earnings results in a negative coefficient whereas not including years of zero earnings results in the positive and significant coefficient. Also, when looking at hourly earnings, the gender gap turns positive, indicating that when the unemployment rate increases, the gender gap in hourly earnings decreases, which the opposite result to that when looking at annual earnings. The results for hourly earnings for both the race and the ethnicity gap are negative, but lack significance. The hourly earnings sample is much smaller than the annual earnings sample, so losing significance for three of the comparisons is not surprising.

The baseline estimate for each of the detailed disability categories (ADL restrictions, IADL restrictions, physical impairments, mental impairments, and sensory impairments) are shown in Table 1.10. These results show that for all of the disability categories except mental impairments, the coefficient of interest is positive and significant, indicating that for each of these disability categories, the gap in annual earnings decreases as the local unemployment rate increases. This is the same result as was seen for work limiting disabilities and difficulties in general. Interestingly, for mental impairments, the coefficient of interest is negative and significant, indicating that the gap in annual earnings increases as local labor market conditions worsen. This pattern of results holds both for annual earnings with years of zero earnings included and for annual earnings without years of zero earnings though for mental impairments, the coefficient of interest is only significant at the 10% level of significance for the regression using annual earnings without years of zero earnings.

The results of the threshold analysis (replace the unemployment rate in equations (1) and (2) with an indicator for the unemployment rate being over 6%) are in Table 1.11. The results are qualitatively very similar to the results in Table 1.9, but the magnitudes of the coefficients are much larger as one would expect. The major differences are that (1) the positive coefficient for the hourly earnings regression for work limiting disabilities is not significant whereas it was in Table 1.9; (2) the positive coefficient for hourly earnings for the gender gap also loses significance compared unlike in Table 1.9; and (3) the two negative coefficients for annual earnings for the ethnicity gap are significant here whereas they were negative, but not significant in Table 1.9.

Tables 1.12-1.14 show the results of the baseline analysis (only using annual earnings for sample size reasons) for each business cycle separately.[18] Table 1.12

[18] I combined the 1980-1981 and the 1981 to 1990 business cycles since some economists do not consider the two recessions in the early 1980s to be separate recessions.

TABLE 1.10: BASELINE REGRESSION RESULTS (Detailed Disability Categories)

WORK LIMITATION

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR	0.065	0.008	0.016
Std. Error	0.014	0.003	0.006
T-Stat	4.71	3.15	2.62
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

ACTIVITIES OF DAILY LIVING RESTRICTION

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR	0.075	0.019	-0.005
Std. Error	0.010	0.002	0.007
T-Stat	7.37	8.52	-0.72
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

INSTRUM. ACTIVITIES OF DAILY LIVING RESTRICTION

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Female * UR	0.064	0.016	0.001
Std. Error	0.010	0.003	0.009
T-Stat	6.10	6.23	0.16
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

PHYSICAL IMPAIRMENT

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Black * UR	0.021	0.010	0.001
Std. Error	0.005	0.001	0.002
T-Stat	4.43	9.48	0.42
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

Table 1.10 Continued

MENTAL IMPAIRMENT	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Hispanic * UR	-0.036	-0.003	-0.001
Std. Error	0.006	0.002	0.003
T-Stat	-5.76	-1.90	-0.37
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

SENSORY IMPAIRMENT	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Black * UR	0.042	0.012	0.006
Std. Error	0.007	0.002	0.003
T-Stat	6.08	7.45	2.10
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

Note: This table shows the coefficient of interest (on the disability/gender/race/ethnicity times unemployment rate variable) as well the associated standard error and t-stat for each regression. Each column is a separate regression where the column heading is the dependent variable. Each panel is the relevant comparison (disability category, gender, race, or ethnicity). Controls include gender, race, ethnicity, marital status, whether s/he has children, education, experience in years, the square of experience in years, industry group, occupation group, state, and year.

TABLE 1.11: REGRESSION RESULTS USING INDICATOR FOR ABOVE OR BELOW 6% UNEMPLOYMENT RATE

DISABILITY (WORK LIMITATION)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR>6	0.318	0.022	0.037
Std. Error	0.053	0.011	0.022
T-Stat	5.94	1.99	1.70
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

DISABILITY (DIFFICULTIES)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR>6	0.107	0.025	0.012
Std. Error	0.023	0.005	0.007
T-Stat	4.72	5.46	1.79
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

GENDER

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR>6	-0.307	-0.040	0.008
Std. Error	0.035	0.009	0.005
T-Stat	-8.82	-4.34	1.49
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

RACE

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR>6	-0.066	0.013	-0.011
Std. Error	0.019	0.006	0.009
T-Stat	-3.52	2.34	-1.32
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

Table 1.11 Continued

	ETHNICITY		
	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Hourly Earnings
Disabled * UR>6	-0.216	-0.019	-0.008
Std. Error	0.064	0.009	0.012
T-Stat	-3.36	-2.07	-0.66
Controls			
N (Person-Years)	5,752,980	4,858,504	380,323
Persons	255,398	255,398	168,816

Note: This table shows the coefficient of interest (on the disability/gender/race/ethnicity times unemployment rate variable) as well the associated standard error and t-stat for each regression. Each column is a separate regression where the column heading is the dependent variable. Each panel is the relevant comparison (disability category, gender, race, or ethnicity). Controls include gender, race, ethnicity, marital status, whether s/he has children, education, experience in years, the square of experience in years, industry group, occupation group, state, and year.

covers the business cycle from 1980 to 1990, Table 1.13 covers 1990 to 2001, and Table 1.14 covers 2001 to 2007. Since macroeconomists have generally found that these business cycles were very different in nature in terms of the roots of the recessions and which segments of the labor force were most affected by the contractions and expansions (Blanchard and Stock, 1986; Stock and Watson, 1996), it would make sense the results shown above might depend on which business cycle is being considered.

Since restricting the number of years cuts the sample size down considerably, many of the coefficients do naturally lose significance. The results do show that there is a good deal of variance across the business cycles though. For example, for annual earnings with zeros included, for work limiting disabilities, the coefficient of interest is negative for 2001 to 2007, but positive for 1990 to 2001. Without years of zero earnings, the above coefficient is positive for 1980 to 1991 and negative for 1991 to 2001. For difficulties, for annual earnings with years of zero earnings, the coefficient is positive for 1980 to 1990 and 1990 to 2001 (though marginally significant), but negative for 2001 and 2007. Also, for the gender comparison, for annual earnings without zeros included, the coefficient is negative for 1980 to 1990, negative though insignificant for1990 to 2001, and positive and significant for 2001 to 2007.

In each of Tables 1.12 – 1.14, in the third column of each panel, I also show the results of regressions estimated on samples that only included those who were in the labor force for the entire period and whose disability status did not change. For the 1980-1990 results, this column looks similar to the column without years of zero earnings though the coefficients are smaller for the disability and gender comparisons. For the 1990-2001 results, the disability comparisons look different from those for the previous business cycle. Here, while the estimates are positive for the first and third

TABLE 1.12: REGRESSION RESULTS FOR 1980-1990

DISABILITY (WORK LIMITATION)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Disabled * UR	0.012	0.015	0.009
Std. Error	0.009	0.004	0.004
T-Stat	1.36	3.90	2.32
Controls			
N (Person-Years)	1,619,412	1,353,557	705,133
Persons	181,733	174,159	64,103

DISABILITY (DIFFICULTIES)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Disabled * UR	0.017	0.010	0.004
Std. Error	0.006	0.002	0.002
T-Stat	3.08	6.32	2.43
Controls			
N (Person-Years)	1,619,412	1,353,557	705,133
Persons	181,733	174,159	64,103

GENDER

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Female * UR	-0.112	-0.014	-0.011
Std. Error	0.007	0.002	0.002
T-Stat	-15.75	-7.26	-6.68
Controls			
N (Person-Years)	1,619,412	1,353,557	705,133
Persons	181,733	174,159	64,103

RACE

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Black * UR	-0.012	0.003	0.000
Std. Error	0.007	0.002	0.003
T-Stat	-1.79	1.31	0.12
Controls			
N (Person-Years)	1,619,412	1,353,557	705,133
Persons	181,733	174,159	64,103

Table 1.12 Continued

ETHNICITY

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Hispanic * UR	-0.055	0.000	-0.002
Std. Error	0.019	0.004	0.004
T-Stat	-2.96	-0.03	-0.54
Controls			
N (Person-Years)	1,619,412	1,353,557	705,133
Persons	181,733	174,159	64,103

Note: This table shows the coefficient of interest (on the disability/gender/race/ethnicity times unemployment rate variable) as well the associated standard error and t-stat for each regression. Each column is a separate regression where the column heading is the dependent variable. The last column only includes those who have positive earnings for the entire period and do not change disability status. Each panel is the relevant comparison (disability category, gender, race, or ethnicity). Controls include gender, race, ethnicity, marital status, whether s/he has children, education, experience in years, the square of experience in years, industry group, occupation group, state, and year.

TABLE 1.13: REGRESSION RESULTS FOR 1990-2001

DISABILITY (WORK LIMITATION)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Disabled * UR	0.141	-0.012	0.013
Std. Error	0.014	0.004	0.006
T-Stat	10.25	-2.67	2.27
Controls			
N (Person-Years)	2,303,818	1,983,712	1,164,600
Persons	227,463	219,118	97,050

DISABILITY (DIFFICULTIES)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Disabled * UR	0.011	0.000	0.006
Std. Error	0.006	0.002	0.002
T-Stat	1.84	0.15	3.11
Controls			
N (Person-Years)	2,303,818	1,983,712	1,164,600
Persons	227,463	219,118	97,050

GENDER

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Female * UR	-0.082	-0.003	-0.001
Std. Error	0.007	0.003	0.003
T-Stat	-11.29	-1.23	-0.49
Controls			
N (Person-Years)	2,303,818	1,983,712	1,164,600
Persons	227,463	219,118	97,050

RACE

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Black * UR	-0.068	0.005	0.007
Std. Error	0.009	0.003	0.003
T-Stat	-7.69	1.95	2.19
Controls			
N (Person-Years)	2,303,818	1,983,712	1,164,600
Persons	227,463	219,118	97,050

Table 1.13 Continued

ETHNICITY

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Hispanic * UR	-0.065	-0.011	-0.016
Std. Error	0.018	0.003	0.003
T-Stat	-3.64	-3.33	-4.84
Controls			
N (Person-Years)	2,303,818	1,983,712	1,164,600
Persons	227,463	219,118	97,050

Note: This table shows the coefficient of interest (on the disability/gender/race/ethnicity times unemployment rate variable) as well the associated standard error and t-stat for each regression. Each column is a separate regression where the column heading is the dependent variable. The last column only includes those who have positive earnings for the entire period and do not change disability status. Each panel is the relevant comparison (disability category, gender, race, or ethnicity). Controls include gender, race, ethnicity, marital status, whether s/he has children, education, experience in years, the square of experience in years, industry group, occupation group, state, and year.

TABLE 1.14: REGRESSION RESULTS FOR 2001-2007

DISABILITY (WORK LIMITATION)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Disabled * UR	-0.084	0.001	-0.004
Std. Error	0.035	0.015	0.018
T-Stat	-2.39	0.08	-0.19
Controls			
N (Person-Years)	1,413,619	1,198,911	920,283
Persons	219,087	202,903	131,469

DISABILITY (DIFFICULTIES)

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Disabled * UR	-0.038	-0.003	-0.002
Std. Error	0.014	0.004	0.003
T-Stat	-2.63	-0.76	-0.62
Controls			
N (Person-Years)	1,413,619	1,198,911	920,283
Persons	219,087	202,903	131,469

GENDER

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Female * UR	-0.018	0.011	0.014
Std. Error	0.008	0.004	0.004
T-Stat	-2.19	2.81	3.42
Controls			
N (Person-Years)	1,413,619	1,198,911	920,283
Persons	219,087	202,903	131,469

RACE

	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Black * UR	-0.002	-0.004	0.005
Std. Error	0.015	0.004	0.005
T-Stat	-0.17	-0.79	1.02
Controls			
N (Person-Years)	1,413,619	1,198,911	920,283
Persons	219,087	202,903	131,469

Table 1.14 Continued

ETHNICITY	Annual Earnings (w/ 0's)	Annual Earnings (w/o 0's)	Annual Earnings (constant sample)
Hispanic * UR	0.017	-0.006	-0.011
Std. Error	0.013	0.007	0.008
T-Stat	1.33	-0.88	-1.42
Controls			
N (Person-Years)	1,413,619	1,198,911	920,283
Persons	219,087	202,903	131,469

Note: This table shows the coefficient of interest (on the disability/gender/race/ethnicity times unemployment rate variable) as well the associated standard error and t-stat for each regression. Each column is a separate regression where the column heading is the dependent variable. The last column only includes those who have positive earnings for the entire period and do not change disability status. Each panel is the relevant comparison (disability category, gender, race, or ethnicity). Controls include gender, race, ethnicity, marital status, whether s/he has children, education, experience in years, the square of experience in years, industry group, occupation group, state, and year.

column, they are negative for the second column for work limiting disabilities and insignificant for difficulties. This is likely related to previous research, which has shown that the 1990s was a time where there were large changes in the labor market outcomes of the disabled and the disability insurance rolls increased dramatically. For 2001-2007, the results in the third column also do not too drastically different from those in the second column. However, for the disability comparisons, the estimates in the first column are now negative whereas they were positive for the previous two business cycles. Also, the estimates are insignificant for the second and third columns whereas the estimates in the third column were positive for the other two business cycles.

These variations are summed up and more clearly seen in the Chart 1.13, a bar chart that shows the coefficient of interest as it changes over time for each regression in each category of comparison. Standard error bars are also included to gauge the significance of each coefficient. Looking at this chart, it is clear that these effects vary from business cycle to business cycle.

Robustness checks were done on the baseline results. These included estimating regressions with leads and lags of the unemployment rate, using SIPP provided weights, using just the DER or the SER variables for the measure of annual earnings, excluding observations where the SSA data indicated that the person was receiving disability insurance payments, applying a best guess for the disability start date for the detailed disability categories, using the state-level employment to population ratio instead of the unemployment rate, and excluding years after an individual in the SIPP from analysis. None of these resulted in estimates that were qualitatively different from those shown in Table 1.9.

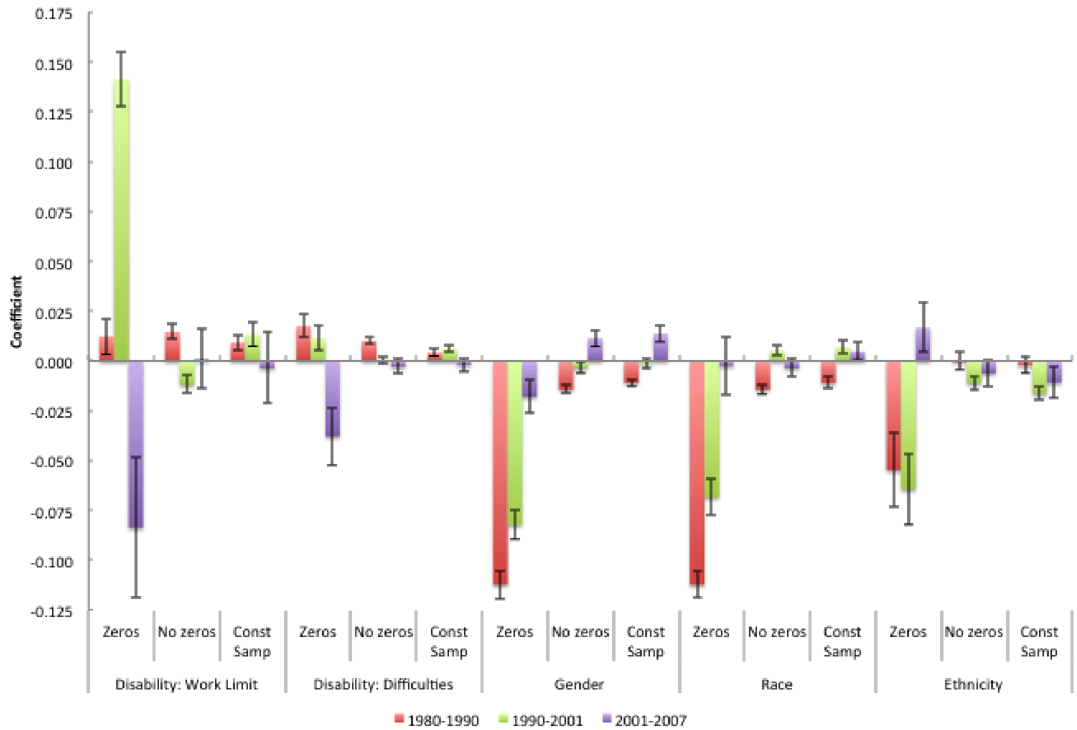

CHART 1.13: BUSINESS CYCLE RESULTS

6. DISCUSSION

This study is the first to study how earnings and wage gaps change over the course of the business cycle for people with different types of disabilities, and for women, blacks, and Hispanics. While the levels of these demographic earnings gaps and the economic disadvantage faced by those with disabilities has been explored in numerous studies and the trends in these levels has also been explored in depth, the cyclicality of these gaps has been studied to a much lesser extent. Building upon Biddle and Hamermesh (2012), I explore how the gaps in annual and hourly earnings between the non-disabled and the disabled, men and women, whites and blacks, and whites and Hispanics changes as the state unemployment rate changes.

Economic theory does not have a clear prediction for whether these earnings gaps should increase as the unemployment rate increases or decreases. An increased ability for employers to discriminate during economic downturns would predict larger earnings gaps as labor market conditions worsen, but self selection among the more "vulnerable" groups (women, blacks, Hispanics, and the disabled) into more stable industries and occupations would predict smaller earnings gaps at higher unemployment rates. Compositional shifts after labor market conditions worsen could affect the results in either direction. This invites an empirical investigation of the question and I use a unique restricted access linked data set to look into this.

My results show that for annual earnings, the disabled seem to fare better than their counterparts as local labor market conditions worsen. For an increase in the unemployment rate of 3.7 percentage points (the average increase in the national unemployment rate from peak to trough during the recessions from 1978 to 2010), the results indicate that gap in annual earnings between those with work limiting disabilities and those without would shrink by 24 percentage points (3 percentage points without years of zero earnings included). For the earnings gap between those with difficulties and those without, the gap would shrink by 8 percentage points (3 percentage points without years of zero earnings).

On the other hand, women seem to fare worse than men. An increase in the unemployment rate of 3.7 percentage points would translate into an increase in the gap in annual earnings of 28 percentage points (2.6 percentage points with years of zero earnings excluded). Blacks seem to do relatively worse when considering both the intensive and extensive margin (3.7 percentage point increase in unemployment rate would mean 8 percentage point increase in annual earnings gap), but slightly better when only considering the intensive margin (same change in unemployment rate would result in 1.5 percentage point decrease in annual earnings gap). The results are largely inconclusive for Hispanics, but evidence from the threshold analysis suggests that they fare relatively worse in recessions. In comparison, Biddle and Hamermesh (2012), looking at weekly earnings, finds that increases in the unemployment rate lead to an increase in the gender gap, a decrease in the race gap, and a small increase in the ethnicity gap.

For hourly earnings, many of the estimates lose statistical significance due to sample size, but there is evidence that the disabled still fare relatively better in recessions (gap in hourly earnings would shrink by 6 percentage points for a increase in the unemployment rate of 3.7 percentage points for work limiting disabilities), as do women (same change in unemployment rate means gender gap in hourly earnings decreases by 1.5 percentage points), but the race and ethnicity gaps increase. My results also show that these effects change depending on which business cycle is being considered.

It is important to remember, when interpreting the results, however, that, as with all studies of earnings and wage gaps, we do not observe the marginal product of labor and thus cannot ascribe a cause to the patterns of earnings gaps across the business cycle. For example, with the disability gap, we do not and cannot know in this study whether taste-based discrimination (or for that matter, a taste for fairness among employers) changes as labor market conditions change or if the pattern of results we see is caused by differential selection into different types of jobs or if the changes in the composition of the disabled population over the business cycle is driving the results or if the main cause is something else entirely. The same applies for the gender, race, and ethnicity earnings gaps as well.

This study has several advantages compared to previous studies, including the fact that I can follow people over a long period of time (1978 to 2010), the larger sample size from pooling nine panels of the SIPP, and that the detailed information on disability in the SIPP allows me to look at a more complete disabled population beyond just those who describe themselves as having a work limiting disability.

There are also several limitations, including the lack of hourly income variables for the years that people are not in the SIPP, the fact that I do not have state of residence, real time reports of disability, and other background information for people when they are not in the SIPP, and the fact that disability is self-reported in the SIPP. Further analysis using other datasets would be useful to determine if these results are affected by the assumptions I make due to data constraints.

This study is among the first to estimate the cyclicality of the disability, gender, race, and ethnicity wage and earnings gaps. It provides evidence that earnings gaps do vary over the business cycle and that the disability, gender, race, and ethnicity gaps are affected in different ways by local labor market conditions. Naturally, it invites further investigation of these and related issues. Foremost among these is the mechanism behind the effects and why these wage and earnings gaps are affectedly differently by labor market conditions. Without additional research, it is impossible to know how to interpret the results from this study and whether the changing wage and earnings gaps come from employer-based discrimination, compositional shifts, workers' preferences, or something else entirely.

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APPENDIX TABLE A1: BASELINE REGRESSION RESULTS WITH MORE DETAILED COEFFICIENTS

DISABILITY (WORK LIMITATION)				DISABILITY (DIFFICULTIES)			
	Co-efficient	Std. Error	T Stat		Co-efficient	Std. Error	T Stat
	Annual Earnings (w/ 0's)				*Annual Earnings (w/ 0's)*		
Disabled	-1.8149	0.084	-21.50	Disabled	-0.4552	0.038	-12.08
UR	-0.0440	0.004	-11.41	UR	-0.0452	0.004	-11.86
Disabled * UR	0.0650	0.014	4.71	Disabled * UR	0.0218	0.006	3.41
Controls				*Controls*			
Person-Years	5,752,980			Person-Years	5,752,980		
Persons	255,398			Persons	255,398		
	Annual Earnings (w/o 0's)				*Annual Earnings (w/o 0's)*		
Disabled	-0.4479	0.016	-27.65	Disabled	-0.1891	0.008	-24.08
UR	-0.0105	0.001	-7.37	UR	-0.0111	0.001	-7.96
Disabled * UR	0.0080	0.003	3.15	Disabled * UR	0.0068	0.001	5.12
Controls				*Controls*			
Person-Years	4,858,504			Person-Years	4,858,504		
Persons	255,398			Persons	255,398		
	Hourly Earnings				*Hourly Earnings*		
Disabled	-0.3116	0.044	-7.14	Disabled	-0.0981	0.010	-9.52
UR	-0.0009	0.002	-0.47	UR	-0.0008	0.002	-0.41
Disabled * UR	0.0160	0.006	2.62	Disabled * UR	0.0026	0.002	1.58
Controls				*Controls*			
Person-Years	380,323			Person-Years	380,323		
Persons	168,816			Persons	168,816		

Table A.1 Continued

	DISABILITY (WORK LIMITATION) GENDER				DISABILITY (DIFFICULTIES) RACE				ETHNICITY		
	Co-efficient	Std. Error	T Stat		Co-efficient	Std. Error	T Stat		Co-efficient	Std. Error	T Stat
	Annual Earnings (w/ 0's)				*Annual Earnings (w/ 0's)*				*Annual Earnings (w/ 0's)*		
Female	-1.0973	0.070	-15.67	Black	0.0686	0.026	2.66	Hispanic	-0.0839	0.144	-0.58
UR	-0.0031	0.008	-0.39	UR	-0.0404	0.004	-10.62	UR	-0.0404	0.004	-10.43
Female * UR	-0.0759	0.013	-5.97	Black * UR	-0.0217	0.004	-5.23	Hispanic * UR	-0.0367	0.024	-1.53
Controls				*Controls*				*Controls*			
Person-Years	5,752,980			Person-Years	5,752,980			Person-Years	5,752,980		
Persons	255,398			Persons	255,398			Persons	255,398		
	Annual Earnings (w/o 0's)				*Annual Earnings (w/o 0's)*				*Annual Earnings (w/o 0's)*		
Female	-0.5860	0.017	-35.11	Black	-0.1219	0.010	-12.66	Hispanic	-0.1023	0.014	-7.08
UR	-0.0069	0.002	-3.05	UR	-0.0107	0.001	-7.65	UR	-0.0101	0.001	-7.05
Female * UR	-0.0072	0.003	-2.44	Black * UR	0.0036	0.002	2.22	Hispanic * UR	-0.0034	0.002	-1.45
Controls				*Controls*				*Controls*			
Person-Years	4,858,504			Person-Years	4,858,504			Person-Years	4,858,504		
Persons	255,398			Persons	255,398			Persons	255,398		
	Hourly Earnings				*Hourly Earnings*				*Hourly Earnings*		
Female	-0.3489	0.010	-35.26	Black	-0.0632	0.014	-4.38	Hispanic	-0.1241	0.017	-7.11
UR	-0.0021	0.002	-1.07	UR	0.0000	0.002	0.00	UR	-0.0001	0.002	-0.06
Female * UR	0.0035	0.002	2.15	Black * UR	-0.0034	0.002	-1.56	Hispanic * UR	-0.0027	0.002	-1.14
Controls				*Controls*				*Controls*			
Person-Years	380,323			Person-Years	380,323			Person-Years	380,323		
Persons	168,816			Persons	168,816			Persons	168,816		

Note: This table shows the results from the same regressions as Table 1.9, but includes more of the estimated coefficients.

CHAPTER 2

IS THE GENDER COMPENSATION GAP SMALLER THAN THE GENDER WAGE GAP?

Chen Zhao[19]

ABSTRACT

One of the explanations sometimes put forth to account for the observed gender gap in wages is that men and women value the nonwage aspects of a job differently. An example is nonwage benefits, which account for about 30 percent of total compensation. If women value nonwage benefits relatively more than men do and there exists a tradeoff between nonwage benefits and wages (as research shows), then the observed gender wage gap would be larger than the gender gap in total compensation. In this paper, I make the first attempt in the economics literature to construct two individual level measures of total compensation in dollar terms – one using supplemental CPS data on employer contribution to health insurance premiums and one using the NLSY linked to employer cost data. The second assumes that the value of nonwage benefits is the industry-and-occupation or the industry-and-firm-size specific cost of providing them. "Total compensation" is then the sum of wages plus contribution towards health insurance premiums for the first measure and the sum of wages plus the cost of benefits an employee is offered for the second measure. I find that the observed gender gap resulting from these measures of total compensation is almost identical to the observed gender gap in wages, providing evidence against the

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idea that the differential valuation of nonwage benefits accounts for some portion of the gender wage gap.

1. INTRODUCTION

The existence of the gender wage gap is well documented in the labor economics literature. Altonji and Blank's 1999 *Handbook of Labor Economics* chapter documents a wage gap in 1995 of about 0.2 log points after controlling for education, experience, industry, occupation, AFQT score, and other demographic characteristics. They describe the gap as "stubbornly persistent." The economics literature has considered several potential sources of the gender wage gap. Two are discussed in Altonji and Blank (1999) -- a difference in human capital accumulation and statistical discrimination. Two others are discussed in Bertrand (2010) -- psychological and attitude differences between men and women that may make some jobs more or less attractive to one gender and the existence of social norms that make some jobs more or less socially "appropriate" for women to do.

Some strands of the literature consider yet another source of the gender wage gap. These studies model each job as a bundle of characteristics that include the offered wage, working conditions, job security, and nonwage benefits. Workers choose whether to accept jobs after taking into consideration all the characteristics (Filer, 1985). The focus of this paper is on nonwage benefits. Even if we ignore and strip away all the other characteristics of a job that may matter to a worker when deciding whether to accept a job, any measure of compensation that does not include nonwage benefits in addition to wages is incomplete.

Almost all of the existing literature on the gender wage gap uses just wages as the dependent variable. Wages, however, only account for about 70 to 75 percent of total compensation costs for most of the past four decades, according to Bureau of

Labor Statistics' Employer Costs of Employee Compensation data. With the recent increase in the cost of health insurance, the proportion of total compensation that is in the form of nonwage benefits has only been increasing and could reasonably be expected to increase more in the future. With this in mind, nonwage benefits are obviously an important omission and, starting with Duncan (1976), the potential importance of nonwage benefits to the wage equations that labor economists estimate has been recognized.

Determining the "value" of total compensation, however, is naturally a difficult exercise. First, there is no obviously correct concept of how the value of nonwage benefits should be measured. Is the value that we are interested in the cost to employers of providing the benefits, the cost workers would incur to provide it for themselves if their employers did not provide the benefits, or is it some intrinsic utility that the worker receives from enjoying the benefit? This difficulty in valuing the benefits is also heterogeneous across different benefits, i.e. health insurance is easier to value than flexible hours. In this study, I use the cost to employers of providing the benefits. Though it may not be the way many would ideally want to measure the value of nonwage benefits, I argue that it is at least one way of measuring the value we are interested in and could be thought of as an approximation of what we are really interested in.

The other difficulty that explains why there are no individual-level datasets that provide valuations in dollar terms of nonwage benefits is that, for at least some benefits, people are often unaware of even whether they have access to the benefit at their jobs, not to mention the cost of providing these benefits, how much it would cost them to provide the benefits for themselves, or how much they "value" it.

The difficulty with finding appropriate data almost certainly explains why so few papers looking at the gender wage gap (or any other topic in labor economics, for that matter) have attempted to take into account total compensation rather than simply wages. There is evidence from the literature, however, to suggest that the gender gap in total compensation is smaller than the gender wage gap. Levy (2006) shows that there is a difference between the gender gap in access to own employer health insurance coverage and the gender wage gap. Using the Current Population Survey, she finds that, for the period 1980 to 2000, the male-female health insurance gap and the black-white health insurance gap are both smaller than the respective wage gaps. This suggests that measuring compensation as wages plus health insurance would result in a smaller compensation gap for men versus women and blacks versus whites than simply using wages.

In addition, some research indicates that some or all of the cost of the employer's contribution to health insurance is passed on to employees in the form of lower wages. Olson (2002) finds that wives with own employer health insurance coverage accept jobs with wages that are 0.2 log points lower. Gruber (1994) uses an exogenous shift in the cost of covering women of childbearing age to show that the full cost of health insurance is shifted to workers in the form of lower wages. And lastly, Baicker and Chandra (2006) estimate that a 10 percent increase in health insurance premiums is associated with a 2.3 percent decrease in wages. This literature indicates that, for health insurance at least, there is tradeoff between wages and access to the benefit.

For other benefits that may be considered "family friendly" benefits, such as flexible hours, child care at work, more vacation/sick days, it is natural to suppose that women value these benefits more than men do and may select into jobs where they have access to these benefits even if the cost is lower wages.

The literature on compensation gaps is sparse, at best. Solberg and Laughlin (1995) use the 1991 wave of the National Longitudinal Survey of Youth, which has data on whether individuals have access to certain benefits at the jobs, to create an index of total compensation using canonical correlation analysis. They conduct their analysis by occupation group and find a significant gender wage gap in six of seven occupational categories, but a significant "compensation" gap in only one of the seven occupational categories. They interpret this as evidence that a gender gap in total compensation would be smaller than the gender wage gap. However, it may be the case that simply looking at the statistical significance of their estimates is not the correct way to interpret their results and it is difficult to compare the point estimates of the coefficients of interest since their compensation index is not in dollar terms.

In contrast to the results in Solberg and Laughlin (1995), Lowen and Sicilian (2009), also using the NLSY, finds that while women are more likely to receive what they term "family-friendly" benefits, they are not more likely to receive other benefits. They also find no evidence that including access to benefits as independent variables changes the coefficient on the gender dummy variable in a wage regression. They interpret this as evidence that the gender wage gap would not be smaller than a gender gap in compensation.

In a study looking at the related black-white wage and compensation gaps, Lepping (2007), using the NLSY, calculates total compensation as the weighted average of wages and the number of fringe benefits received. Here, while the wage gap is found to be significant, the compensation gap is not and this is interpreted as evidence that the black-white compensation gap is smaller than the black-white wage gap.

To actually answer the question of what would happen to the gender wage gap if total compensation is used in place of wages, one would ideally like to simply do just that -- redo the analysis done in the gender wage gap literature with a different dependent variable. In this paper, I attempt to use that approach by constructing two measures of total compensation using different methods. First I use the Current Population Survey to construct a measure of compensation that is simply wages plus employers' contribution to health insurance. While this is far from ideal and fails to take into account the myriad of other nonwage benefits, it does account for one of the costliest components of nonwage benefits for employers. This can be thought of as a first step toward answering the question of what would happen to the gender wage gap if the cost of nonwage benefits were included.

Second, I use data on employers, the Bureau of Labor Statistics' Employer Costs of Employee Compensation dataset, in conjunction with individual level data from the National Longitudinal Survey of Youth. I link the industry and occupation as well as industry and firm size specific costs of providing nonwage benefits to individual level data on access to these nonwage benefits in order to place a value on the benefits that is equal to the cost of providing them.

I then re-estimate the standard gender wage gap equations using my two constructed measures of compensation and find that there is no difference between the gender gap in total compensation and the gender wage gap. While these two measures are far from perfect, these results do provide evidence that differential valuation of nonwage benefits by men and women may not account for any portion of the observed gender wage gap.

The rest of this paper proceeds as follows: second two details the various data sources I used, section three explains the methods I used to link the data sources and do the analysis, section four contains the results, and section five concludes.

2. DATA

2.1 *Current Population Survey*

The Current Population Survey (CPS) is a largely cross-sectional monthly survey conducted by the Census Bureau for the Bureau of Labor Statistics to assess the employment situation and is commonly used in gender wage gap papers. The CPS started in 1940 and surveys about 60,000 households (120,000 individuals) chosen from the civilian non-institutional population each month. Households are in the CPS for a total of eight months (four months continuously in, eight months out, and four months in) so short panels can be formed by linking households from month to month. The CPS contains labor market information for each member of household in the outgoing rotation group 15 years of age or older in the calendar week containing 12th day of the month. The CPS also conducts special supplements certain months collecting additional data based on certain topics. The most regular supplement is the March Annual Social and Economic Supplement (ASEC) that includes data on income received in the previous calendar year and health insurance coverage. Since the mid 1980's the BLS has also been conducting an Occupational Mobility and Job Tenure Supplement in January or February.

As my first measure of compensation, I use the 1995 and 2005 February and March Current Population Surveys. I chose 2005 both because I wanted this section to be comparable to the later analysis I do with the 2006 and 2008 National Longitudinal Survey of Youth and 2006 and 2008 Employer Costs of Employee Compensation data and because 2005 is the most recent Occupational Mobility and Job Tenure Supplement to have been conducted in February (linking from February to March allows for a larger linked sample than linking January to March). 1995 was chosen as robustness check to make sure that the results are not unique to just one year of data.

I use both the February and March Supplements because the March supplement has labor market information, whether one has own employer health insurance coverage, and how much one's employer contributes toward health insurance premiums, but not whether one is eligible for own employer health insurance coverage. Using just the March supplement would not allow me to identify those who were offered health insurance by their employers, but declined to take it up. The February supplement, however, does have a question on whether one is offered health insurance from one's employer, which allows me to identify everyone who is eligible for health insurance from his/her employers. Taking advantage of the quasi-panel nature of the CPS, I link the two supplements together (described in section three) for my analysis.

2.2 *National Longitudinal Survey of Youth*

The National Longitudinal Survey of Youth 79 (NLSY79) is a panel survey of individuals born between the years 1957 and 1964. From 1979 to 1994, these individuals were interviewed annually and since 1994, they have been interviewed biannually. Originally, there were 12,686 individuals, aged 14-22 years, in the survey in 1979. Over time however, there has been much attrition and only about 7,000 were interviewed in 2006 and 2008. The NLSY has also commonly been used in gender wage gap studies since it allows researchers to control for many variables not found in the CPS, including the AFQT (Armed Forces Qualification Test) score, which is often used as a proxy for ability (Neal and Johnson, 1996; Rodgers and Spriggs, 1996; Strain, 2011).

Apart from a large number of demographic and family background variables and labor market information, the NLSY79 also includes a variable that indicates whether the individual is offered the following benefits at each job s/he currently holds: health insurance, life insurance, dental insurance, retirement, sick days, vacation days, maternity leave, profit sharing, training, flexible hours, and child care. I use these variables on access to nonwage benefits in conjunction with data on the cost of offering nonwage benefits from the Employer Costs for Employee Compensation data to create a second measure on the value of total compensation.

2.3 *Employer Costs of Employee Compensation Data*

The Employer Costs for Employee Compensation (ECEC) is a dataset provided by the Bureau of Labor Statistics (BLS) and is derived from the National Compensation Survey (NCS), a quarterly survey of approximately 4,000 employers (employing about 14 million workers) in the US. The dataset starts in 1986 and covers both private and state/local government. The ECEC reports, by industry, occupation, firm size, region and union status, the cost to employers in dollars per hour of wages and salaries as well as various nonwage benefits (aggregated at different levels in different extracts of the data). These benefits include paid leave, supplemental pay, insurance, retirement, and legally required benefits.

For the years 2006 to 2011, the BLS publishes supplemental tables that provide the same benefit cost data but reported by the following cross tabulations: industry and occupation as well as industry and firm size. These are the measures of nonwage benefit costs that I will link to the NLSY data on individual access to nonwage benefits by industry and occupation as well as industry and firm size.

3. METHODS

3.1 Data Linkages

3.1.1 *Linking the February and March CPS Data Sets*

Because of the quasi-panel nature of the CPS, I was able to link the February and March supplements for 2005 and 1995. Households in the CPS are interviewed for four consecutive months, then not interviewed for eight consecutive months, and then interviewed again for another four consecutive months for a total of eight months that they are in the survey. So, for example, if a household is interviewed from January to April of 2005, then it would be interviewed again from January to April of 2006, but it would not be in the May to December 2005 CPS datasets. When a household is in month four or month eight (April 2005 and April 2008 in the example), they are in the so-called "Outgoing Rotation Group."

With this structure, in theory, 75 percent of the households in the February and March supplements could be linked. This figure is less than 100 percent because those in month four or month eight of the interview cycle in the February supplement would not be in the March supplement and those in month one or month five of the interview cycle in the March supplement would not be in the February supplement. Only those in months one through seven in the February supplement and therefore, in months two through eight in the March supplement could potentially be in my sample.

Ideally, person level identifiers would be used to link the two supplements. In reality, since the CPS public use datasets do not contain person level identifiers, I linked individuals in the two supplements by household identifier, month in sample, person line number, family number, state, sex, and race. Those observations that were found to be duplicates based on the above variables were dropped.

3.1.2 *Linking the NLSY to the ECEC*

I created two linkages between the NLSY and the ECEC: first, by industry and occupation specific cells and second, by industry and firm size specific cells. Unfortunately, the NLSY and the ECEC do not use the same codes for industry and occupation. While the ECEC uses Census codes for both industry and occupation, the NLSY uses NAICS (North American Industry Classification) codes and SOC (Standard Occupational Classification) codes. I used published crosswalks to harmonize the two sets of industry and occupation classification systems.[20] Due to confidentiality concerns, the ECEC data is only cross-tabulated at the two digit industry and occupation level.

To create the first linkage, I first put all individuals in the NLSY into industry and occupation specific cells. Then, for each benefit the individual has access to at their primary job (Job #1 in the NLSY, also known as the "CPS job"), I code the value of the benefit as the average industry and occupation specific cost to employers of providing the benefit according to the ECEC data. To create the second linkage using industry and firm size rather than industry and occupation, I followed basically the same procedure as above. First, I placed all individuals from the NLSY into industry and firm size category cells and assigned each individual, for each benefit available to him, the average industry and firm size specific cost to employers of providing the benefit using the ECEC data.

It is important to note that I was not able to use as many individuals in the NLSY as I would have liked due to the fact that I was using published public-use cross tabulations of the ECEC data. Some of the industry by occupation cells or industry by firm size category cells in the ECEC were so small that the BLS could not publish

[20] The Census Bureau maintains crosswalks between these industry and occupations codes. See: http://www.census.gov/hhes/www//ioindex/crosswalks.html

them without risk to confidentiality protection, so only a subset of the cells are published. I used two methods to deal with this issue. First, I simply dropped those individuals in the NLSY belonging to industry and occupation cells that I did not have cost data for from the analysis. As a second method, I used multiple imputation to impute the cost data for the industry/occupation and industry/firm size cells that I did not have data for.

Multiple imputation is an alternative to mean imputation and conditional mean imputation, which produce too many cases at the mean, produce estimates that are biased, and, mostly importantly, reduces variance because the imputed values contain no error. Standard analysis using singly imputed values will overstate the significance of the parameter estimates since the standard errors will be too small. In contrast, multiple imputation is done by drawing multiple values from a distribution, meaning the imputed values inherently contain some variance. Analysis is done separately on each “completed” dataset with each imputed value and the separate estimates are pooled to generate a single set of results. The final parameter estimate is simply the mean of all the parameter estimates obtained from each imputation. The final multiple imputation estimate of the standard error is the square root of the sum of the within imputation variance (the average of the variance across the imputations) and the between imputation variance (a function of the variance of the parameter estimate across the imputed datasets and the number of imputations). By using all of the results across the imputations, multiple imputation accounts for the uncertainty in the imputed values (Meng, 1995; Rubin, 1996; Little and Rubin, 2002; Graham, et al. 2007).

For this study, I imputed the cost of each of the benefits for the industry/ occupation and the industry/firm size cells that were suppressed in the ECEC data. The variables used in my imputation model are: hourly wage, race, ethnicity, education, potential experience, region, SMSA status, industry group, occupation group, establishment size, marital status, whether the individual has children, and union status. These include all the variables in the regression equations I estimate in my analysis. Since the missingness pattern in my data was not monotonic, I used both the multivariate normal model (drawing from a multivariate normal distribution of all the variables in the imputation model) and the chained equations approach (generates imputations by performing a series of univariate regressions). As results from the two approaches were very similar, I only show results from the chained equations approach. The results shown are also estimated using ten implicates for the imputed variables. Results using five, twenty, and fifty implicates were qualitatively identical.

Another data issue is that, in terms of firm size categories for the industry by firm size category cross tabulation, the BLS designed the coarseness of the firm size categories for each industry so as to not run into small cell issues. This means that while for some industries, firm size categories were 1-49, 50-100, 100-199, 200-499, and 500+, for other industries, some of these had to be combined, making for coarser cells.

Lastly, the NLSY also did not use the same benefit categories as the ECEC uses for its cross tabulations. The benefit categories are shown in Table 2.1. I assigned individuals who had access to medical insurance, dental insurance, and life insurance the cell specific cost of providing insurance in the ECEC and I assigned individuals who had access to vacation days, sick days, and maternity leave the cell specific cost of providing paid leave. There was a high of degree of correlation between having access to one type of insurance and another and having one type of paid leave and another. I also ran the analysis only requiring individuals to have access to have one type of insurance or one type of paid leave to be assigned the cost of insurance or paid leave and found that it did not make a difference to the main results.

TABLE 2.1: BENEFIT CATEGORY LINKAGES

Employer Costs for Employee Benefits	National Longitudinal Survey of Youth
Insurance	Medical Insurance
	Dental Insurance
	Life Insurance
Paid Leave	Vacation Days
	Sick Leave
	Maternity Leave
Retirement	Retirement
Supplemental Pay	Profit Sharing

*3.2 **Analysis***

3.2.1 CPS Analysis

For the purpose of analysis, I dropped those individuals who, at the time of the survey, were in school or the army, those who were not employed and working full time for the whole year, and those who were working for the government. I also, dropped the top and bottom two percent of the wage distribution.

There are two measures of hourly wage in the CPS. Everyone in the Outgoing Rotation Group (ORG) has a measure of hourly wage. Since only a third of my linked sample is in the ORG in March, this severely limits my sample size. The March supplement also has a measure of annual wages last year and hours worked last year, so an hourly wage variable can be constructed for everyone in the linked sample who worked last year. I conducted my analysis using both of these hourly wage measures: the self-reported hourly wage variable for everyone in the ORG and the constructed hourly wage variable for everyone in both the February and March supplements.

The two compensation variables for the CPS sample were created by summing each of the two hourly wage variables with the individual's employer contribution toward health insurance premiums if the individual was offered health insurance at his job.

Since not everyone who is offered health insurance by his employer takes it up (resulting in a health insurance contribution of zero dollars by the employer in the observed data), but the value of the employer contribution toward premiums had he taken up the offer of health insurance should be counted toward his total compensation, I had to impute the value of employer contribution toward health insurance premiums for those were offered, but did not take up. This was also done by using multiple imputation. The variables I used in my imputation model were: hourly wage, race, ethnicity, potential experience, education, region of residence, industry group, occupation group, marital status, and whether the individual has children less than six years of age. As with the NLSY-ECEC cost imputations, I also used ten implicates to produce the CPS results.[21]

With two hourly wage variables and two compensation variables (constructed from the two wage variables) for each individual, I then ran the standard wage regressions using each of the dependent variables:

$$(1) \qquad ln(HourlyWage_i) = \beta_1 + \beta_2 Male_i + \beta_3 X_i + \varepsilon_i$$

$$(2) \qquad ln(HourlyCompensation_i) = \beta_1 + \beta_2 Male_i + \beta_3 X_i + \varepsilon_i$$

where X_i is a vector of control variables including demographic characteristics and industry/occupation depending on the specification. I ran three different specifications, each with a different set of control variables. The first model

[21] Results produced from using five, twenty, and fifty implicates are not shown, but were qualitatively identical.

controlled for race, ethnicity, education, potential experience, and region of residence; the second controlled for the aforementioned variables as well as industry group, occupation group, and firm size; and the third controlled for all the aforementioned variables as well as marital status and whether the individual has a child less than six years of age.

3.2.2 ***NLSY Analysis***

The NLSY analysis was done is a very similar manner. Here I dropped those that were not interviewed in 2006 and 2008, those currently enrolled in school, those currently in the armed forces, those not employed or employed by non-traditional employers, those employed by the government, those not in the labor force for the entire year, those who did not work at least 35 hours per week, those with self-reported negative hourly pay, and those who did not answer the benefit questions. I also trimmed the top and bottom two percent of the wage distribution. These cuts to the data and how many observations were dropped are shown in Table 2.9.

In Table 2.9, I also show how many additional observations are dropped if those in the suppressed cost cells (in the ECEC) are dropped. Since, the sample size with the suppressed cells imputed is twice as large for 2008 and more than three times as large for 2006 as the sample size with the suppressed cells dropped, I will show estimate my results both for the sample with the suppressed cells dropped and for the sample with the cost variables in the suppressed cells imputed.

The total compensation measure for the NLSY was created by summing hourly wage for the main CPS job and the cell specific costs of each benefit the individual was offered at his main job. The regression analysis was done in the same way as for the CPS with the only difference being slightly different control variables. As with the CPS analysis, I estimated three different specifications. The first controlled for race, ethnicity, education, potential experience, region of residence, and SMSA status; the second controlled for these variables as well as industry group, occupation group, and establishment size; and the third controlled for all of these variables as well as marital status, whether the individual has children, and union status. All of the regression analyses were done using sampling weights provided by the NLSY.

Lastly, as a robustness check, I redid all of the regression analyses using just the sample of individuals who reported being married in the relevant year, 2006 or 2008, and found that this did not make a qualitative difference in the results.[22]

4. RESULTS

4.1 *CPS Results*

The summary statistics for the CPS samples I used - the Outgoing Rotation Group and the entire linked February/March supplements- are shown in Tables 2.2 (for 1995) and 2.3 (for 2005). Not surprisingly, the statistics across the two samples look very similar even though the Outgoing Rotation Group sample is just under a third the size of the entire linked sample.

Each group is roughly evenly divided between the genders and the average age is about 38 years for the 1995 data and 41 years the 2005 data. These statistics show that there are more black females than black males and more Hispanic males than Hispanic females in all samples. The males are more likely to be married and have young children present in the household in all samples. In the 1995 data, males and females are about equally likely to have a college degree, but in the 2005 data, more females have a college degree. The distribution across firm size looks pretty similar in

[22] One might think that married men and women are more likely differentially value nonwage benefits (especially family-friendly ones) than non-married men and women. The results (tables available from author upon request) show that this is not is the case in these data.

all samples, but women are more likely to be employed by the largest employers (those with more than 1000 employees).

TABLE 2.2: SUMMARY STATISTICS FOR THE CPS (1995)

	OUTGOING ROTATION GROUP			MARCH SUPPLEMENT		
	All	**Male**	**Female**	**All**	**Male**	**Female**
		49.2%	50.8%		50.5%	49.5%
Age	38.6	38.5	38.7	39.0	39.1	38.9
	(12.5)	(12.3)	(12.4)	(12.4)	(12.4)	(12.3)
Black (%)	9.21	7.95	10.44	8.44	7.13	9.77
Hispanic (%)	6.64	7.09	6.20	6.19	6.81	5.55
Married (%)	60.12	63.88	56.48	61.59	65.56	57.54
Have Children <6 (%)	18.11	19.87	16.41	17.80	19.21	16.36
College Educated (%)	27.53	27.10	27.95	27.78	27.37	28.18
Firm Size 1-9 (%)	11.75	11.38	12.10	14.53	15.10	13.96
Firm Size 10-24 (%)	8.75	9.79	7.75	8.98	9.66	8.30
Firm Size 25-99 (%)	13.20	13.83	12.56	13.41	14.14	12.67
Firm Size 100-499 (%)	16.10	16.18	16.02	15.22	14.89	15.56
Firm Size 500-999 (%)	6.81	6.19	7.41	6.41	5.67	7.16
Firm Size 1000+ (%)	43.39	42.64	44.13	41.44	40.54	42.36
Hourly Wage	$12.30	$13.90	$10.80	$13.20	$15.20	$11.20
	($7.10)	($7.80)	($6.00)	($8.40)	($9.30)	($6.90)
Hourly Compensation	$13.50	$15.20	$11.90	$14.30	$16.40	$12.20
	($7.80)	($8.20)	($7.20)	($9.10)	($10.00)	($7.60)
Employer Contribution to Health Insurance	$2,988	$3,325	$2,587	$3,031	$3,386	$2,586
	($1425)	($1499)	($1217)	($1479)	($1567)	($1223)
Have Health Insurance	77.48	77.06	77.88	76.80	76.25	77.35
Health Insurance from Employer	56.74	62.27	51.37	53.43	57.51	49.28

TABLE 2.2: Continued

		OUTGOING ROTATION GROUP			MARCH SUPPLEMENT		
		All	**Male**	**Female**	**All**	**Male**	**Female**
			49.2%	50.8%		50.5%	49.5%
No Employer HI	Employer Offers Health Ins	16.68	11.88	21.33	15.90	11.13	20.76
	Eligible for Emp Health Ins	10.03	6.91	13.06	9.37	6.54	12.25
Employer Health Insurance	Emp. Pay All of Premium	18.11	19.16	17.10	16.22	17.03	15.40
	Emp. Pays Part of Premium	35.01	39.12	31.01	33.88	36.96	30.74
	Emp. Pays None of Premium	1.86	1.99	1.73	1.75	1.77	1.73

Note: Does not include imputed values. Values in parenthesis are standard deviations.

TABLE 2.3: SUMMARY STATISTICS FOR THE CPS (2005)

	OUTGOING ROTATION GROUP			MARCH SUPPLEMENT		
	All	**Male**	**Female**	**All**	**Male**	**Female**
		49.8%	50.2%		50.8%	49.2%
Age	41.1	40.8	41.4	41.4	41.3	41.4
	(13.0)	(13.0)	(12.9)	(13.1)	(13.1)	(13.0)
Black (%)	8.47	7.42	9.51	8.03	6.91	9.18
Hispanic (%)	10.13	11.78	8.49	9.57	10.79	8.31
Married (%)	58.22	61.82	54.65	59.43	63.27	55.46
Have Children <6 (%)	16.14	17.66	14.62	15.79	17.08	14.46
College Educated (%)	29.99	28.28	31.68	30.62	28.73	32.57
Firm Size 1-9 (%)	13.26	13.26	13.60	15.79	16.68	14.86
Firm Size 10-24 (%)	10.12	11.25	9.00	10.05	10.75	9.32
Firm Size 25-99 (%)	13.69	14.62	12.77	13.43	14.11	12.73
Firm Size 100-499 (%)	14.91	14.96	14.86	14.21	14.19	14.24
Firm Size 500-999 (%)	6.00	5.16	6.83	5.67	5.20	6.16
Firm Size 1000+ (%)	42.02	40.75	43.27	40.85	39.07	42.69
Hourly Wage	$17.70	$18.70	$15.60	$18.40	$20.70	$16.10
	($15.80)	($10.90)	($9.20)	($12.70)	($14.00)	($10.70)
Hourly Compensation	$19.00	$20.60	$17.30	$20.10	$22.50	$17.60
	($13.50)	($11.70)	($14.90)	($13.70)	($14.90)	($11.80)
Employer Contribution to Health Insurance	$4,873	$5,386	$4,288	$4,922	$5,475	$4,265
	($2326)	($2371)	($2128)	($2372)	($2426)	($2129)
Have Health Insurance	75.58	74.45	76.70	75.77	74.87	76.71
Health Insurance from Employer	54.60	57.82	51.41	52.15	54.68	49.54

TABLE 2.3: Continued

		OUTGOING ROTATION GROUP			MARCH SUPPLEMENT		
		All	**Male**	**Female**	**All**	**Male**	**Female**
			49.8%	50.2%		50.8%	49.2%
No Employer HI	Employer Offers Health Ins	17.60	14.16	20.99	16.80	13.22	20.49
	Eligible for Emp Health Ins	11.43	9.75	13.09	10.77	8.88	12.73
Employer Health Insurance	Emp. Pay All of Premium	11.48	12.29	10.68	10.99	11.47	10.50
	Emp. Pays Part of Premium	39.31	41.38	37.25	37.69	39.53	35.79
	Emp. Pays None of Premium	1.43	1.59	1.28	1.51	1.53	1.49

Note: Does not include imputed values. Values in parenthesis are standard deviations.

For both years of data, men have a higher average hourly wage and a higher hourly compensation using the measure of total compensation I constructed compared to women. Turning to benefits, women are slightly more likely to have health insurance from any source, but men are more likely to have health insurance coverage from their own employer. This indicates that women more likely to get spousal coverage from their husbands' jobs, which is consistent with the findings in other studies (Buchmueller, 1996). Of those without own employer health insurance coverage, women are more likely to be both working in a firm that offers health insurance and be eligible for that health insurance. In other words, again, women are more like to not take up offered health insurance and get it from another source. These summary statistics seem to indicate that men and women may have roughly the same rate of access to health insurance from their employers (similar to results in Levy, 2006). Certainly, the gender gap in access to this nonwage benefit is much smaller than the wage gap, but the employer contribution to health insurance for males is higher than for females so the gender gap in the value of this benefit is higher than the gender gap in access. Of course, it is well documented in the literature that females take up health insurance less than males do because their husbands cover the whole family (Buchmueller, 1996-1997). This is a plausible and likely driver of the higher value of employer health insurance contribution for males.

For 1995, the results from estimating equations (1) and (2) are in Table 2.4 for the Outgoing Rotation Group sample and in Table 2.5 for the entire linked sample and, for 2005, the results are in Tables 2.6 and 2.7. The tables show the results for three different specifications, each with a different set of control variables, which are listed in the tables. The results across all three of these specifications, across the two samples, and across the two years of data show the same basic qualitative result. There is no appreciable difference between the gender gap in wages and the gender

TABLE 2.4: 1995 CPS RESULTS (OUTGOING ROTATION GROUP)

	Model 1		Model 2		Model 3	
	Hourly Wage	Compensation	Hourly Wage	Compensation	Hourly Wage	Compensation
Male	0.2281	0.2273	0.1628	0.1591	0.1605	0.1557
	(0.0080)	(0.0081)	(0.0084)	(0.0085)	(0.0084)	(0.0084)
N	11,721	11,721	11,721	11,721	11,721	11,721
Controls						
Black	yes	yes	yes	yes	yes	yes
Hispanic	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes
Pot. Experience	yes	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes	yes
Industry Group			yes	yes	yes	yes
Occupation Group			yes	yes	yes	yes
Firm Size			yes	yes	yes	yes
Married					yes	yes
Child < Age 6					yes	yes

Note: Includes 1,446 imputed values of employer contribution to health insurance premium.

TABLE 2.5: 1995 CPS RESULTS (MARCH SUPPLEMENT)

	Model 1		Model 2		Model 3	
	Hourly Wage	**Compensation**	**Hourly Wage**	**Compensation**	**Hourly Wage**	**Compensation**
Male	0.2722	0.2686	0.2056	0.2019	0.2007	0.1961
	(0.0056)	(0.0056)	(0.0060)	(0.0060)	(0.0060)	(0.0060)
N	37,105	37,105	37,105	37,105	37,105	37,105
Controls						
Black	yes	yes	yes	yes	yes	yes
Hispanic	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes
Potential Experience	yes	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes	yes
Industry Group			yes	yes	yes	yes
Occupation Group			yes	yes	yes	yes
Firm Size			yes	yes	yes	yes
Married					yes	yes
Child < Age 6					yes	yes

Note: Includes 4,309 imputed values of employer contribution to health insurance premium.

TABLE 2.6: 2005 CPS RESULTS (OUTGOING ROTATION GROUP)

	Model 1		**Model 2**		**Model 3**	
	Hourly Wage	**Compensation**	**Hourly Wage**	**Compensation**	**Hourly Wage**	**Compensation**
Male	0.1987	0.2006	0.1622	0.1615	0.1567	0.1533
	(0.0079)	(0.0081)	(0.0084)	(0.0085)	(0.0084)	(0.0084)
N	12,481	12,481	12,481	12,481	12,481	12,481
Controls						
Black	yes	yes	yes	yes	yes	yes
Hispanic	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes
Potential Experience	yes	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes	yes
Industry Group			yes	yes	yes	yes
Occupation Group			yes	yes	yes	yes
Firm Size			yes	yes	yes	yes
Married					yes	yes
Have Child < Age 6					yes	yes

Note: Includes 1,657 imputed values of employer contribution to health insurance premium.

TABLE 2.7: 2005 CPS RESULTS (MARCH SUPPLEMENT)

	Model 1		Model 2		Model 3	
	Hourly Wage	**Compensation**	**Hourly Wage**	**Compensation**	**Hourly Wage**	**Compensation**
Male	0.2576	0.2528	0.2208	0.2148	0.2116	0.2036
	(0.0055)	(0.0056)	(0.0059)	(0.0060)	(0.0059)	(0.0060)
N	39,944	39,944	39,944	39,944	39,944	39,944
Controls						
Black	yes	yes	yes	yes	yes	yes
Hispanic	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes
Potential Experience	yes	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes	yes
Industry Group			yes	yes	yes	yes
Occupation Group			yes	yes	yes	yes
Firm Size			yes	yes	yes	yes
Married					yes	yes
Have Child < Age 6					yes	yes

Note: Includes 5,001 imputed values of employer contribution to health insurance premium.

gap in compensation when compensation is measured as wages plus employer contribution toward health insurance premiums.

For both 1995 and 2005, the gender gaps in wages and compensation for the Outgoing Rotation Group sample are smaller across all three specifications than the gender gaps for the entire linked sample. This likely results from the fact that the hourly wage variable is self-reported in the Outgoing Rotation Group sample while it is calculated from self-reported annual wages and self-reported total hours worked in the larger linked sample. There is some research on the difference between these two CPS hourly wage variables in the literature that finds that there is less measurement error in the Outgoing Rotation Group self-reported hourly wage variable (Liu, 2009).

4.2 *NLSY and ECEC Results*

Summary statistics for the 2006 and 2008 ECEC data that I used are shown in Table 1.8. Wages are, on average, across all industries, occupations, and firm sizes, about 70 percent of total compensation. The most expensive benefit category, aside from legally required benefits (not shown), is insurance. The main component of insurance is health insurance, which is about just over 8 percent of total compensation. Paid leave, including vacation days, sick days, and maternity leave, are a close second at 7 percent of total compensation after insurance. Retirement and savings (4.4 percent) and supplemental pay (2.5 percent) are much smaller categories in terms of cost to employers.

Summary statistics for the NLSY samples I used are shown in Table 2.10 and 2.11. The samples within a year (2006 or 2008) are different because these tables only include the observations that did not require imputation of benefit costs. As discussed in the methods section, some industry by occupation cells and industry by firm size cells were dropped due to the unavailability of ECEC data.

TABLE 2.8 COMPENSATION STATISTICS

		Dollar Amount	Percentage
2006	Total Compensation	$27.54	100.0%
	Wages and Salaries	$19.24	69.9%
	Total Benefits	$8.30	30.1%
	Paid Leave	$1.91	7.0%
	Supplemental Pay	$0.69	2.5%
	Insurance	$2.26	8.2%
	Retirement and Savings	$1.21	4.4%

		Dollar Amount	Percentage
2008	Total Compensation	$28.87	100.0%
	Wages and Salaries	$20.37	69.8%
	Total Benefits	$8.81	30.2%
	Paid Leave	$2.03	7.1%
	Supplemental Pay	$0.74	2.5%
	Insurance	$2.45	8.4%
	Retirement and Savings	$1.29	4.4%

TABLE 2.9: SAMPLE SIZES

	2006		2008	
CUT TO SAMPLE	N	%	N	%
Drop non-interview	6,793	100%	7,596	100%
Currently enrolled in school	6,611	97%	7,193	95%
Currently in armed forces	6,591	97%	7,181	95%
Drop if not employed or non-traditional employers	4,718	69%	5,298	70%
Drop government workers	5,362	79%	5,215	69%
Drop if not in labor force for entire year	4,501	66%	4,333	57%
Drop if not worked at least 35 hours	3,873	57%	3,732	49%
Drop if hourly pay is negative	3,788	56%	3,654	48%
Drop if valid skip on benefits	3,788	56%	3,646	48%
Drop bottom 2% and top 2% of earnings distribution	3,634	53%	3,491	46%
Drop if compensation cells missing	919	14%	1,463	19%

TABLE 2.10: SUMMARY STATISTICS FOR THE NLSY (2006)

	INDUSTRY OCCUPATION CELLS			INDUSTRY FIRMSIZE CELLS		
	All	Male	Female	All	Male	Female
		50.3%	49.7%		55.8%	44.2%
Age	44.7	44.8	44.6	44.7	44.7	44.6
	(2.2)	(2.2)	(2.3)	(2.2)	(2.2)	(2.3)
Black (%)	30.26	26.50	34.1	29.20	26.26	32.92
Hispanic (%)	17.88	17.75	18.01	17.97	18.24	17.63
Married (%)	58.69	64.28	52.87	59.18	64.14	52.90
Have Children (%)	80.29	78.38	83.27	79.67	79.33	82.36
College Educated (%)	25.21	27.13	23.25	25.70	26.35	24.89
Estab Size 1-49 (%)	34.24	33.25	35.25	36.34	36.74	35.83
Estab Size 50-99 (%)	13.01	12.62	13.41	13.44	13.22	13.73
Estab Size 100-499 (%)	26.66	26.38	26.95	27.72	27.14	28.46
Estab Size 500+ (%)	21.79	24.38	19.16	22.50	22.91	21.99
Union Member (%)	18.32	20.50	16.09	20.09	21.78	17.63
Hourly Wage	$20.00	$23.50	$16.60	$20.70	$23.30	$17.50
	($13.00)	($14.90)	($9.70)	($12.90)	($14.10)	($10.20)
Hourly Compensation	$22.70	$26.60	$18.70	$23.30	$26.20	$19.70
	($15.30)	($17.40)	($11.60)	($15.00)	($16.50)	($11.90)
Have Insurance (%)	82.12	82.75	81.48	81.98	81.59	82.48
Have Paid Leave (%)	52.87	49.88	55.94	53.27	49.87	57.59
Have Profit Sharing (%)	33.99	35.25	32.69	24.12	33.48	34.93
Have Retirement Benefits (%)	84.14	85.00	83.27	83.31	83.35	83.26

Note: Does not include imputed values. Values in parenthesis are standard deviations.

TABLE 2.11: SUMMARY STATISTICS FOR THE NLSY (2008)

	INDUSTRY OCCUPATION CELLS			INDUSTRY FIRMSIZE CELLS		
	All	Male	Female	All	Male	Female
		45.5%	54.5%		52.0%	49.4%
Age	46.6	46.6	46.7	46.6	46.6	46.7
	(2.3)	(2.3)	(2.3)	(2.3)	(2.3)	(2.3)
Black (%)	29.26	27.78	30.49	28.49	27.09	29.96
Hispanic (%)	17.24	16.54	17.82	17.35	17.56	17.12
Married (%)	58.31	62.99	54.41	58.61	62.84	54.16
Have Children (%)	80.05	77.95	81.8	79.57	77.91	81.32
College Educated (%)	27.57	27.67	27.49	27.53	26.93	28.15
Estab Size 1-49 (%)	35.55	34.42	36.49	37.62	37.16	37.37
Estab Size 50-99 (%)	13.25	12.37	13.98	13.26	12.88	13.66
Estab Size 100-499 (%)	27.01	27.78	26.36	28.41	27.95	28.89
Estab Size 500+ (%)	20.41	23.38	18.01	21.07	22.01	20.08
Union Member (%)	21.02	21.82	20.36	21.63	22.48	20.74
Hourly Wage	$21.98	$24.69	$18.63	$22.13	$24.69	$19.42
	($12.13)	($13.75)	($9.79)	($12.30)	($13.56)	($10.14)
Hourly Compensation	$24.06	$27.84	$20.90	$24.78	$27.72	$21.69
	($14.28)	($16.30)	($11.43)	($14.32)	($15.87)	($11.72)
Have Insurance (%)	81.69	82.34	81.14	82.25	81.73	82.8
Have Paid Leave (%)	51.82	50.06	53.28	53.08	51.05	55.23
Have Profit Sharing (%)	30.9	32.62	29.46	31.25	32.16	30.29
Have Retirement Benefits (%)	84.65	83.91	85.27	84.66	83.37	86.01

Note: Does not include imputed values. Values in parenthesis are standard deviations.

Some of the samples are evenly split across gender, but two of the samples are skewed and this is probably an artifact of the trimming that I had to do due to the ECEC data available. The NLSY samples (at about 44.7 years in 2006 and 46.7 years in 2008) are on average older than the CPS samples, but this has to do with the design of the NLSY. Again, the proportion black among females is higher than the proportion black among males. Men are again more likely to be married, but women are more likely to have children in the household. In the 2006 samples, men are more likely to have a college degree, but in 2008, they are not. This difference is again probably due to people shifting industries, occupations, or firms and which industry/occupation or industry/firm-size cells I had to drop. The distribution across firm sizes is fairly similar across gender with men slightly more likely to be in firms with more than 500 employees. I note these results may seem different from those obtained from the CPS samples, but these are different size categories than those used for the CPS samples (where the largest category was 1000+) and firm size was looked at in the CPS samples whereas establishment size is the variable here. Lastly, men are more likely to be in unions, especially in the 2006 samples, probably due to the occupations they are in.

Again, the average hourly wage among men is higher than among women and the same can be said about the average calculated hourly compensation variable. The distribution in access to insurance (health insurance, life insurance, and dental insurance) at one's job is similar across the gender, but women are more likely to be offered paid leave (vacation days, sick days, and maternity leave) in all samples. Men seem generally more likely to have access to profit sharing. Men also seem slightly more likely in 2006 to have access to retirement benefits, but, for 2008, women seem more likely to have access to retirement benefits. Again, here the gender gap in access to benefits seems to be much smaller than the gender wage gap.

Table 2.12 has the same summary statistics for both the 2006 and 2008 Industry/Firm Size NLSY samples by whether the individuals were missing ECEC cost data (i.e. required imputation of cost data). From this table, we can see that observations requiring imputation were different from those not requiring imputation in terms of the following observable characteristics: gender, race, education, establishment size, union status, hourly wages, and access to benefits. These differences indicate that simply dropping these observations may bias the final estimates.

Table 2.13 shows the probabilities of having access to a particular benefit category both conditional on having access to another category of benefits and the unconditional probability. The take-away from this table is that the probability of having access to a benefit conditional on having access to another benefit is higher than the unconditional probability for all the benefits, but that the conditioning on any of the other benefit categories results in a similar probability. Also, the conditional probabilities for paid leave and profit sharing are far from 100%.

Results from estimating equations (1) and (2) on the NLSY samples are shown in Table 2.14 for the industry occupation cells sample and in Table 2.15 for the industry firm size cells sample for 2006 and Tables 2.16 and 2.17 for 2008. Each table shows the coefficient on the male dummy variable from estimation on the sample where suppressed ECEC cells were dropped and the sample where the benefit costs for these cells were imputed using multiple imputation (highlighted in grey).

The main qualitative result here is the same as the results from the CPS samples. The estimated gender gaps in compensation are virtually identical to the estimated gender wage gaps in all of the samples across both years and across all three

TABLE 2.12: SUMMARY STATISTICS FOR THE MISSING CELLS				
	2006		**2008**	
	Non Missing	Missing	Non Missing	Missing
Male (%)	50.3%	62.1%	55.8%	65.0%
Age	44.7	44.7	44.7	40.8
	(2.2)	(2.2)	(2.2)	(2.2)
Black (%)	30.26	28.37	29.20	28.20
Hispanic (%)	17.88	20.30	17.97	19.99
Married (%)	58.69	59.39	59.18	58.21
Have Children (%)	80.29	81.55	79.67	74.92
College Educated (%)	25.21	21.02	25.70	20.44
Estab Size 1-49 (%)	34.24	45.07	36.34	46.15
Estab Size 50-99 (%)	13.01	10.48	13.44	11.78
Estab Size 100-499 (%)	26.66	22.91	27.72	39.64
Estab Size 500+ (%)	21.79	18.55	22.50	16.93
Union Member (%)	18.32	24.91	20.09	26.05
Hourly Wage	$20.00	$20.09	$20.70	$23.33
	($13.00)	($16.97)	($12.90)	($14.61)
Hourly Compensation	$22.70		$23.30	
	($15.30)		($15.00)	
Have Insurance (%)	82.12	76.14	81.98	74.92
Have Paid Leave (%)	52.87	52.18	53.27	52.04
Have Profit Sharing (%)	33.99	25.75	24.12	23.44
Have Retirement Ben. (%)	84.14	78.61	83.31	77.86

Note: Values in parenthesis are standard deviations.

TABLE 2.13: BENEFIT CATEGORY CROSS TABULATIONS

2006 NLSY

Benefit	Insurance	Paid Leave	Profit Share	Retirement
Overall	78.5%	51.9%	29.3%	80.7%
Insurance		61.0%	34.2%	92.6%
Paid Leave	92.2%		35.5%	93.2%
Profit Share	91.5%	62.9%		95.0%
Retirement	90.1%	60.0%	34.5%	

2008 NLSY

Benefit	Insurance	Paid Leave	Profit Share	Retirement
Overall	78.3%	51.7%	27.4%	81.3%
Insurance		60.1%	32.1%	93.0%
Paid Leave	91.0%		31.7%	92.8%
Profit Share	91.8%	59.7%		93.5%
Retirement	89.6%	59.1%	31.5%	

Note: This table reports the percent of observations that are in column category conditional on being in row category. Also, these samples include those observations that required imputation of benefit costs.

TABLE 2.14: 2006 NLSY RESULTS (INDUSTRY OCCUPATION CELLS)

	Model 1		Model 2		Model 3	
	Hourly Wage	**Compensation**	**Hourly Wage**	**Compensation**	**Hourly Wage**	**Compensation**
Male	0.2661	0.2656	0.2260	0.2249	0.2169	0.2144
	(0.0340)	(0.0346)	(0.0353)	(0.0357)	(0.0354)	(0.0358)
N	919	919	919	919	919	919
Male	0.2307	0.2176	0.2231	0.2237	0.2045	0.2041
	(0.0181)	(0.0188)	(0.0200)	(0.0204)	(0.0203)	(0.0207)
N	3,634	3,634	3,634	3,634	3,634	3,634
Controls						
Black	yes	yes	yes	yes	yes	yes
Hispanic	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes
Potential Experience	yes	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes	yes
SMSA	yes	yes	yes	yes	yes	yes
Industry Group			yes	yes	yes	yes
Occupation Group			yes	yes	yes	yes
Establishment Size			yes	yes	yes	yes
Married					yes	yes
Have Children					yes	yes
Union					yes	yes

Note: The highlighted cells include the following number of imputed values: 1,549 for insurance benefits, 1,597 for retirement benefits, 1064 for paid leave benefits, and 520 for supplemental pay benefits.

TABLE 2.15: 2006 NLSY RESULTS (FIRM SIZE CELLS)

	Model 1		Model 2		Model 3	
	Hourly Wage	**Compensation**	**Hourly Wage**	**Compensation**	**Hourly Wage**	**Compensation**
Male	0.2211	0.2186	0.2080	0.2054	0.1925	0.1887
	(0.0247)	(0.0257)	(0.0264)	(0.0269)	(0.0264)	(0.0269)
N	1,192	1,192	1,192	1,192	1,192	1,192
Male	0.2307	0.2213	0.2231	0.2232	0.2045	0.2037
	(0.0181)	(0.0189)	(0.0200)	(0.0204)	(0.0203)	(0.0207)
N	3,634	3,634	3,634	3,634	3,634	3,634
Controls						
Black	yes	yes	yes	yes	yes	yes
Hispanic	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes
Potential Experience	yes	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes	yes
SMSA	yes	yes	yes	yes	yes	yes
Industry Group			yes	yes	yes	yes
Occupation Group			yes	yes	yes	yes
Establishment Size			yes	yes	yes	yes
Married					yes	yes
Have Children					yes	yes
Union					yes	yes

Note: The highlighted cells include the following number of imputed values: 1,173 for insurance benefits, 1,230 for retirement benefits, 815 for paid leave benefits, and 361 for supplemental pay benefits.

TABLE 2.16: 2008 NLSY RESULTS (INDUSTRY OCCUPATION CELLS)

	Model 1		**Model 2**		**Model 3**	
	Hourly Wage	**Compensation**	**Hourly Wage**	**Compensation**	**Hourly Wage**	**Compensation**
Male	0.2940	0.2959	0.2290	0.2280	0.2218	0.2205
	(0.0228)	(0.0240)	(0.0239)	(0.0246)	(0.0238)	(0.0246)
N	1,140	1,140	1,140	1,140	1,140	1,140
Male	0.2720	0.2581	0.2418	0.2424	0.2304	0.2308
	(0.0171)	(0.0179)	(0.0186)	(0.0191)	(0.0185)	(0.0190)
N	3,491	3,491	3,491	3,491	3,491	3,491
Controls						
Black	yes	yes	yes	yes	yes	yes
Hispanic	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes
Potential Experience	yes	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes	yes
SMSA	yes	yes	yes	yes	yes	yes
Industry Group			yes	yes	yes	yes
Occupation Group			yes	yes	yes	yes
Establishment Size			yes	yes	yes	yes
Married					yes	yes
Have Children					yes	yes
Union					yes	yes

Note: The highlighted cells include the following number of imputed values: 1,292 for insurance benefits, 1,343 for retirement benefits, 903 for paid leave benefits, and 395 for supplemental pay benefits.

TABLE 2.17: 2008 NLSY RESULTS (FIRM SIZE CELLS)

	Model 1		**Model 2**		**Model 3**	
	Hourly Wage	**Compensation**	**Hourly Wage**	**Compensation**	**Hourly Wage**	**Compensation**
Male	0.2690	0.2702	0.2433	0.2410	0.2344	0.2320
	(0.0201)	(0.0212)	(0.0220)	(0.0223)	(0.0220)	(0.0223)
N	1,463	1,463	1,463	1,463	1,463	1,463
Male	0.2722	0.2670	0.2421	0.2437	0.2306	0.2322
	(0.0171)	(0.0180)	(0.0186)	(0.0189)	(0.0185)	(0.0189)
N	3,491	3,491	3,491	3,491	3,491	3,491
Controls						
Black	yes	yes	yes	yes	yes	yes
Hispanic	yes	yes	yes	yes	yes	yes
Education	yes	yes	yes	yes	yes	yes
Potential Experience	yes	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes	yes
SMSA	yes	yes	yes	yes	yes	yes
Industry Group			yes	yes	yes	yes
Occupation Group			yes	yes	yes	yes
Establishment Size			yes	yes	yes	yes
Married					yes	yes
Have Children					yes	yes
Union					yes	yes

Note: The highlighted cells include the following number of imputed values: 836 for insurance benefits, 884 for retirement benefits, 594 for paid leave benefits, and 220 for supplemental pay benefits.

specifications with different sets of control variables. Whether benefit costs for the suppressed ECEC cells were imputed also does not make a difference to the result qualitatively. In comparison to the CPS results, the estimated gender gaps in all of these NLSY samples are similar in magnitude to the gender gaps estimated for the entire linked March supplement samples, but larger than the gender gaps estimated for the Outgoing Rotation Group samples.

For 2006, the estimated gender gaps for the industry/occupation group NLSY sample are slightly larger than for the estimated gender gaps for the industry/firm-size NLSY sample while 2008 is more of a mixed bag. This can probably be explained by which cells were dropped due to lack of ECEC data. The main result, the absence of any appreciable difference between the gender gap in my constructed measure of total compensation and the gender gap in hourly wages, holds for all samples, however.

5. DISCUSSION

The gender wage gap is well documented in the labor economics literature and one of the possible explanations sometimes put forth to account for some of this observed gap in wages is that women may value more highly other aspects of compensation, such as access to employer provided health insurance, more paid leave, flexible hours, or child care at work. In a society where women spend more time on childcare activities and assume more of the responsibility of childbearing, it is natural to expect that women would value some of these family friendly benefits more than men. Also, women with children might be more interested in making sure the family has access to health insurance, the cost of which (the sum of the employee's own contribution toward premiums and any lost wages to pay for the employer's portion) is much cheaper in the group market in which employers procure health insurance than on the individual market. The difference in cost is due to both dispersion of risk in the group market and the relatively favorable tax treatment of employer provided health insurance.

The lack of work on this issue stems most likely from the difficulty in measuring the value of nonwage benefits and the resulting lack of data. In this paper, I take two approaches: (1) measuring compensation as the sum of wages and employer contributions to health insurance premiums in CPS data and (2) assuming that the value of various nonwage benefits is the industry and occupation or the industry and firm size specific cost of providing them and measuring compensation as the sum of wages plus the cost of benefits individuals in the NLSY have access to at their jobs. My results show virtually no difference between the gender gap in hourly wages and the gender gap in my constructed compensation measures across all the samples I used.

While this paper is the first to attempt to directly construct measures of total compensation in dollar terms at the individual level and estimate gender gaps using these measures, it does have several shortcomings, mostly stemming from data issues. One of these is the many missing industry/occupation and industry/firm size cells in the ECEC data, which forces me to drop many observations in my NLSY samples or impute benefit costs for these cells. Another problem is that the CPS only has information on health insurance benefits, which comprises a large component of nonwage benefits, but is still only one benefit. In addition, since males are more likely to cover their families with the health insurance plans offered at their jobs, their employers are more likely to contribute to a more expensive family plan. Thus, the value of that benefit for men is relatively inflated. Ideally, what we would like to use is the maximum amount that the employer would contribute for health insurance premiums for a given individual.

The use of industry and occupation specific or industry and firm size specific cost data is also problematic because (1) it conceals much firm to firm variation in costs (using the public use version of the ECEC data actually exacerbates this problem because of the coarseness of the industry and occupation cells in the data) and (2) cost to the firm tells you little about person to person variation in intrinsic valuation of benefits. My second measure of total compensation assumes that the value of nonwage benefits is the cost to employers of providing them. Of course, the value of benefits is vague and could be defined in many ways and there are many reasons to think that the cost of provision under- or over-approximates the true value of the benefits to individual employees (i.e. health insurance is cheaper in the group market than in the individual market, some employees may not value a particular benefit at all that every employee at the firm has access to because of the nature of the benefit, etc.).

Though I would argue that my approach approximates the value of total compensation and is the first attempt that I know of to put a dollar value at the individual level on total compensation and provides meaningful evidence on the question of whether the gender compensation gap is smaller than the gender wage gap, the shortcomings discussed above could explain the similarity between the estimated gender gaps in my results.

Future work could benefit from addressing the weaknesses of this study by using finer cost data on benefits or trying to use other approximations, at the individual level, of the value of nonwage benefits. These could include surveying people and asking how much they value benefits in terms of lost wages or determining how much it would cost for an employee to provide the benefit for himself. In addition, future research would estimate other wage gaps, such as the race and ethnicity gaps, using total compensation measures derived using similar methods.

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CHAPTER 3
HEALTH ENDOWMENT AND PARENTAL INVESTMENT IN SIBLINGS

Chen Zhao[23]

ABSTRACT

Economists have long been interested in how parents allocate scarce resources among children with different levels of initial endowment. If parents are interested in maximizing the return on their investment, they might reinforce initial conditions, but if parents are more motivated by equity, then they might compensate for initial differences. The existing literature shows evidence of both types of behavior. Either would have implications for studies that use sibling fixed effect models and assume no differential treatment by parents of siblings with different health endowments. I use six panels of the SIPP (1992-2008) to see how differences in health endowments between siblings affect parental investment in children in various age groups. Unlike previous studies, I directly measured health endowment as whether the child has any health conditions or disabilities. I measure parental investment as the frequency with which parents do the following with each child: read, play or going on fun outings, eat breakfast, eat dinner, praise, etc. Consistent with previous studies, the results show that there is some evidence that parents do not invest equally in children of different health endowments for at least some of the investments considered, but the evidence is

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far from overwhelming. Moreover, the pattern of parental investment appears to differ depending on education level of the parents and the general age range of the children. In general, these results seem to indicate that whether parents engage in reinforcing or compensatory behavior and, if so, which depends crucially on the specific investment. This is consistent with the heterogeneity in the results obtained in previous studies.

1. INTRODUCTION

The economics literature has competing theories about how parent allocate resources between and invest in children with different levels of initial endowment with both reinforcement of and compensation for initial differences as possibilities. A desire to maximize the total welfare of the family might lead to reinforcement of initial differences as the expected marginal return of investment in the more endowed child is higher compared to investment in the lesser-endowed child. (Becker and Tomes, 1976) However, parents might be more interested in equalizing differences between siblings and invest more in the lesser-endowed child even if the expected return on the investment is lower. (Behrman, Pollack, and Taubman, 1982) The pattern of behavior that parents exhibit, whether it is reinforcing or compensating, has important policies implications as well as implications for empirical studies that estimate the effect of initial health conditions on short and long term outcomes using sibling fixed effect models since these studies assume no differential treatment from parents (examples include: Currie and Stabile, 2006; Fletcher, 2013).

Broadly defining endowments as inherited initial traits that directly or indirectly after future attainment, many studies have empirically tested whether parental investments are affected by each child's initial endowment. The results have not converged on a consensus. While some have found no effect of child endowment on parental investment decisions (Del Bono et al, 2008; Royer, 2009; Kelly, 2009; Almond and Currie, 2011), some have found that parents compensate for initial differences (Behrman, Pollak, and Taubman 1982; Griliches 1979) and others have found evidence of reinforcing behavior among parents (Behrman, Rosenzweig, and Taubman 1994; Datar, Kilburn, and Loughran 2010; Rosenzweig and Wolpin 1998; Rosenzweig and Schultz 1982). In addition, Hsin (2012) finds no evidence of compensation or reinforcement overall, but, after looking by education level, finds that less educated mothers exhibited reinforcing behavior while more educated mothers behaved in the opposite way, compensating for initial differences.

This study uses the 1992-2008 panels (six in total) of the Survey of Income and Program Participation (SIPP) to look at how parents invest, in terms of time and resources, in siblings with different health endowments. Health endowment is determined using the SIPP's Functional Limitation and Disabilities – Children topical module, which asks the designated parent (this is usually the mother) whether each child has any of a long list of age-appropriate disabilities, conditions, and disorders. Children who have any of these are categorized as having low level of health endowment (bad health) in this study and children who do not have any of the conditions, disorders, and limitations in the list are categorized as having a high level of health endowment (good health). Parental investment variables come from the SIPP's Children's Well-Being topical module. The designated parent is asked a long list of questions about how often they, the father, and all adults do various activities with each of the children in the family. These include: reading, playing, praising, eating breakfast and dinner, going on fun outings, etc. I examine the pattern of behavior exhibited by parents, defined in terms of the frequency of these activities, in siblings with different health endowments using family fixed-effect models.

The results show that, for some of these activities, parents do invest differently in siblings that have different levels of health endowments. This offers evidence that parents do compensate or reinforce initial differences and would indicate that studies using sibling fixed effect models to estimate the effect of childhood health conditions on long-term outcomes may be over- or under-stating the true effects. However, for most of the investment outcomes, there is no evidence that parents exhibit either reinforcing or compensatory behavior. Furthermore, the actual direction of effect, whether parents compensate or reinforce initial differences in endowments seems to depend on the specific investment being considered. This is consistent with the previous literature as a whole and indicates that the reason for the disparate results in previous empirical studies is the heterogeneity in the measures of parental investment that are being used across studies. Consistent with Hsin (2012), the results also offer some evidence that parents' behavior may vary depending on the general age range of the children as well as the parents' level of education.

This study contributes to the existing empirical literature on this topic in several ways. First, rather than using a proxy for parental contributions (such as adult outcomes) or looking at the raw amount of time that parents spend with children, I am looking at actual meaningful activities that parents do with their children and I am able to look at the frequency with which each parent engages in each activity with the each child. These activities include reading, eating breakfast and dinner, going on fun outings, and offering praise. Studies have shown that a parent performing these types of activities with children enhances their cognitive home environment and positively affects later life outcomes. (Brooks-Gunn and Markman 2005; Davis-Kean 2005, Smith et al. 1997) While the frequency of these activities is not a novel measure of parental investment, the only other study that I am aware of that uses similar measures was done using British data.

In addition, I also measure health endowment in a different and arguably, more meaningful, way than previous studies. Rather than using a proxy for initial endowment or using birth weight, I use whether the child has any actual condition or disability. I argue that siblings that differ from each other in the sense that one has a health condition or disability and the other does not is more salient to parents as a difference in health endowments than a simple difference in birth weight. This is especially true if the difference in birth weight is not large and neither sibling would be considered to be of "low birth weight."

Another advantage of this study compared to many of the previous studies is that by using six panels of the SIPP, I have a considerably larger total sample size and, more importantly, larger sample of siblings that actually differ in health status. Lastly, a concern with empirical studies in this literature is that siblings are observed at different ages, which may account for any differences in observed parental investment. I put the children in my sample into different age groups (0-6, 6-11, and 6-14) so that all the siblings being compared are in the same general age group. By narrowing the age range of the siblings that are being compared, this partially mitigates the concern that differences in parental investment are attributable to differences in age.

The next section will give some background on the competing theoretical models and existing empirical literature, section three will talk about the data and methods I am using, section four presents the empirical results, and the last section is a discussion of the results.

2. BACKGROUND AND LITERATURE

Many empirical health economics papers estimating the long-term outcomes of childhood health conditions use sibling fixed-effect models and assume that parents invest in children with different health endowments equally. However, the theoretical literature in economics has long argued that parents respond to differences in initial endowments in siblings and allocate resources accordingly, depending on preferences for equality and resource constraints.

Becker and Tomes (1976, 1986) introduced a model in which parents do not favor any one of the children in their utility function (child-neutral) and are concerned with maximizing the total wealth of each child. Parents invest in each child's human capital until the marginal return on human capital equals the return on financial assets and then parents allocate transfers so as to offset differences in earnings between children. In this model, if the return on investment in the more endowed child is greater, then parents would adopt a reinforcing strategy or invest more in the more endowed child. If the opposite is true, then parents may adopt a compensating strategy.

The "separable earnings-transfer" (SET) model introduced by Behrman et al. (1982) posits that parents have preferences for the distribution of earnings ability among their children, not just total wealth (which also includes transfers from parents). Here, depending on initial endowments among children, parental preferences for equality versus productivity, and the actual rates of return on investment in each child, parents may adopt reinforcing, neutral, or compensating strategies.

In addition, Conley (2008) proposed that parental allocation decisions may vary by socio-economic class. In particular, when resources are more limited, parents may be more risk averse and the least risky strategy may be a reinforcing one if the return on investment in the more endowed child is greater. In other words, equality may be a luxury good that only the well-off can afford and the less well-off may not be able to afford to spread resources among all children since that may dilute resources to the point of failing to provide any of the children with the means to succeed.

While these models help to identify factors that may drive parents' decisions, ultimately, the question of how parents allocate resources to siblings with different health endowments is an empirical one. Studies have yielded mixed results. Griliches (1979) finds evidence of compensatory behavior among parents when using adult IQ as a proxy for initial endowment. Specifically, the effects of IQ are found to be smaller among sibling pairs than across individuals and the differences decrease as the siblings become more alike in terms of age, gender, and genetics. Likewise, Behrman et al (1982) estimates the parental preference parameters of a SET model and also finds that parents compensate for initial differences in a sample of adult male twins. However, Behrman et al. (1994) finds that schooling attainment was higher for the more endowed child using adult earnings and body mass index as proxies for investment and endowments and interprets this as evidence of reinforcing behavior.

Unfortunately, these older studies all used indirect measures of both initial endowments and parental investments and, therefore, require somewhat dubious assumptions. For example, using educational attainment as a proxy for parental investments assumes that educational attainment is only affected by parental investment and not initial endowments and preferences. Also, using adult earnings as a proxy for initial endowment assumes that, for example, family environment does not affect adult labor market outcomes.

More recent studies have used birth weight as a more direct measure of initial health endowment as well as more direct measures of parental investment. Almond and Currie (2011) uses the Early Childhood Longitudinal Study-Birth Cohort (ECLS-B) to look at outcomes such as breastfeeding, well-baby visits, amount of praise and affection offered, and disciplinary practices and find no evidence that parents treat their low birth weight babies differently from their non-low birth weight babies. These results are very similar to those found in Royer (2009), which also used the

ECLS-B and finds that measures of neonatal medical care (breastfeeding, NICU admission, etc.) did not vary within twins with differences in birth weight.

Two other studies that find no evidence of differences in parental investment are Kelly (2009), which finds no investment responses (i.e. time spent reading to child, etc.) from parents after Asian flu-induced damages in a 1958 British birth cohort study, and Del Bono (2008), which finds that models that allow the endowment of siblings to affect parental investment in an index child result in very similar estimates compared to mother fixed effect models. Del Bono (2008) does find a significant difference for breastfeeding, but the magnitude of the effect is very small.

Datar et al. (2010) looks at siblings in the National Longitudinal Survey of You – Child (NLSY-C) to look at a similar set of outcomes to Almond and Currie (2011) and Royer (2009). While a continuous measure of birth weight yields insignificant estimates, they find evidence of reinforcing behavior among parents when using a dummy variable for low birth weight status. However, because of the outcomes they look at (breastfeeding, well-baby visits, immunizations, attend preschool), the results they find could be due to poorer health in low birth weight children (i.e. less healthy children may be seeing specialists for their conditions and less likely to be taken in for routine well baby visits). The authors also look at how the presence of low birth weight siblings on outcomes for normal birth weight children and only find a significant effect for well baby visits, but this result could be due to transaction costs (i.e. the cost of caring for a low birth weight sibling may lessen the resources available left to care for the normal birth weight sibling).

Hsin (2012) uses the Child Development Supplement of the Panel Study of Income Dynamics (PSID-CDS) to look at how much time parents spend with each of their children and measures health endowment by birth weight. She finds no evidence of either compensation or reinforcement overall, but after taking into consideration the socio-economic background of the families, she finds that while less educated parents spend more time with their heavier weight children (reinforcing behavior), more educated parents exhibit the opposite behavior (compensating behavior). She finds this to be true of both total time spent with children as well as time spent doing educational activities.

In addition to these studies using US data, a number of studies have found evidence of reinforcing behavior in parental investments across siblings in developing countries (Ayalew 2005; Pitt et al. 1990; Rosenzweig and Schultz 1982; Rosenzweig and Wolpin 1988). As these are developing countries, these results would be consistent with those in Hsin (2012) for less educated parents.

3. DATA AND METHODS

The data used for this study comes from the Survey of Income and Program Participation (SIPP). The SIPP is a longitudinal survey that is administered in panels where each panel is a short panel (usually 2-4 years). This study uses the 1992, 1993, 1996, 2001, 2004, and 2008 panels, each of which consisted of 20,000-50,000 households (the later panels were larger). Within each panel, the SIPP is conducted in multiple waves (usually 9-13 waves) where each wave consists of a set of core questions that remain the same for each wave and a set of questions from a topical module that changes for each wave.

For this study, I use the Children's Well-Being topical module and the Functional Limitations and Disabilities – Child topical module, both of which are administered at least once during each of the 1992-2008 panels. When the topical modules were administered more than once within a panel, I used answers from the earliest instance of the topical module to maximize the sample size.[24] Since the set of questions that I am interested in changes depending on the general age range of the child for both of the topical modules that I use,[25] I do my analysis by three different age groups: 0-6, 6-11, and 6-14. Because of differences in structure between the 1992-1993 and 1996-2008 panels, the 1992-1993 panels are only represented in the 0-6 age group and not the 6-11 or 6-14 age groups.[26] The division into age groups is explained in detail below.

3.1 *Health Endowment Measure*

The variables used to create the measure of health endowment come from the Functional Limitations and Disabilities – Children topical module. This topical module asks the designated parent (usually the mother) a series of questions about each child in the family. These questions differ depending on the age of the child being asked about.

For children age 0-6 in the 1992 and 1993 panels, parents are asked whether the child has any developmental conditions requiring therapy or diagnostic services and whether the child has any long lasting conditions that limit his ability to walk, run, or use stairs. If parents answer yes to either question, I put the child in the poor health endowment group. Otherwise, I put the child in the good health endowment group. For children age 0-6 in the 1996 – 2008 panels, parents are only asked whether the child has any conditions that limit ordinary activities. For this age group in these panels, if parents answer yes to this question, I put the child in the poor health

[24] Sample sizes for later waves are usually smaller than for earlier waves as the incidence of non-responses increases.

[25] That is, each question is only asked of children in a specific age range, i.e. age 6-11, and this range depends on the specific question.

[26] The Child Well-Being topical module for the 1992-1993 panels did not contain the outcomes measures I use for the age 6-11 and age 6-14 age groups.

endowment group and if the parents answer no, then the child is put in the good health endowment group.

For children aged 6-14, parents are asked a long list of questions regarding each child's health. These questions cover the following conditions, limitations, and disorders:

- Learning disabilities
- Mental retardation
- Developmental disability
- ADHD
- Any other developmental condition requiring therapy or diagnostic services
- Cane, crutches, or walker
- Wheelchair or electric scooter
- Hearing aid
- Difficulty hearing
- Difficulty seeing words, letters, with glasses
- Difficulty having speech understood
- Long-lasting condition that limits ability to walk, run, participate in sports
- Difficulty getting around inside house
- Difficulty getting in or out of bed or chair
- Difficulty taking bath or shower
- Difficulty putting on clothes
- Difficulty eating food
- Difficulty getting to or using toilet
- Emotional or mental condition cause difficulty getting along with other children
- Condition that limits ability to do regular school work
- Ever received special education services

Since these questions are asked about children age 6 to 14, I use the same measure of health endowment for the 6-11 and 6-14 age groups. For both age groups, if the child has any of the above conditions, disabilities, or difficulties, then the child is placed in the poor health endowment and children that do not have any of the above are coded as having good health endowment.[27]

This mapping of variables from the SIPP by panel and age group is summarized in Table 3.1.

3.2 Parental Investment Measures

The measures of parental investment come from the Children's Well-Being topical module of the SIPP. The designated parent is asked a series of questions about how often various activities are done with each child. As with the health questions from the Functional Limitations and Disabilities topical model, different questions are asked of children in different age groups. Thus, not all of the parental investment outcomes were used for each age group. Table 3.2 shows which investments outcomes were considered for each of the three age groups. In summary, all of the investments were used for the age 0 to 6 age group and only four of the investments were used for the age 6 to 11 age groups while the rest were used for the age 6 to 14 age group.

3.3 Analysis

For reasons explained above, the sample was divided into three different age groups: 0-6, 6-11, and 6-14. In each, I only keep those families with more than one child in the age group. Sets of siblings where the children are not all in the same age

[27] As robustness checks, I conducted all of my analysis using different ways of specifying the bad_health variable. In one, I only coded children who had more than one of the conditions, disabilities, or difficulties as having bad health. In another, I changed the threshold to more than two of the conditions, disabilities, and difficulties. In a third, I only considered health conditions. The results from these additional analyses did not differ qualitatively from those presented in Tables 5 – 7.

TABLE 3.1: HEALTH VARIABLES USED FOR EACH AGE GROUP

Age 0-6		Age 6-11	Age 6-14	Health Variable
1992-1993 Panels	1996-2008 Panels			
x				Developmental condition requiring therapy or diagnostic services
x				Long lasting condition that limits ability to walk, run, or use stairs
	x			Any conditions that limit ordinary activities
		x		Learning disabilities
		x		Mental retardation
		x		Developmental disability
		x		ADHD
		x	x	Any other developmental condition requiring therapy or diagnostic services
		x	x	Cane, crutches, or walker
		x	x	Wheelchair or electric scooter
		x	x	Hearing aid
		x	x	Difficulty hearing
		x	x	Difficulty seeing words, letters, with glasses
		x	x	Difficulty having speech understood
		x	x	Long-lasting condition that limits ability to walk, run, participate in sports
		x		Difficulty getting around inside house
		x		Difficulty getting in or out of bed or chair
		x		Difficulty taking bath or shower
		x		Difficulty putting on clothes
		x		Difficulty eating food
		x		Difficulty getting to or using toilet
		x		Emotional or mental condition cause difficulty getting along with other children
		x		Condition that limits ability to do regular school work
		x		Ever received special education services

TABLE 3.2: INVESTMENTS CONSIDERED FOR EACH AGE GROUP

Age 0-6	Age 6-11	Age 6-14	Outcome (Number of Times)
x*	x		all people read to child
x	x		father read to child
x	x		designated parent read to child
x*	x		any kind of outing
x		x	designated parent ate breakfast with child
x		x	father ate breakfast with child
x		x	designated parent ate dinner with child
x		x	father ate dinner with child
x		x	designated parent spent more than 5 min w child for fun
x		x	father spent more than 5 min w child for fun
x		x	designated parent praised child
x		x	father praised child

Note: * Includes 1992, 1993, 1996, 2001, 2004, and 2008 panels. All others only include 1996, 2001, 2004, and 2008 panels.

group are excluded from the analysis samples. A benefit of this strategy is that this allows for the comparison of siblings that are closer in age. A potential issue that one may be concerned with in a study like this is that we may observe parents investing differently across siblings because the children may be of very different ages at the time of the survey. Only considering siblings that are all in the same age group partially mitigates these concerns.[28]

In addition, I transform the outcomes for each child such that it is a measure of the deviation, for that child, from the mean outcome for his/her age and adjusting for the standard deviation. Specifically, I subtract the average value for the child's age of the investment outcome from the value for the specific child and then divide the

[28] Some may argue that the age ranges are rather large and include children at different stages of development, but these age ranges are narrower than what has been used in previous studies.

difference by the standard deviation for the age. By doing this, the outcomes are an age-standardized measure of the outcome for each child.

While the outcome measures for the bulk of the outcomes were continuous variables, I transformed the following outcomes measures to be binary variables indicating whether the frequency for the child was above or below the age-specific average: designated parents spent more than 5 minutes with child for fun, father spent more than 5 minutes with child for fun, designated parent praised child, father praised child.[29]

For each of the investment outcomes and for each of the age groups, I estimate the following family fixed-effect model:

$$I_{im} = \beta_0 + \beta_1 e_{im} + \beta_2 X_{im} + \tau_m + \varepsilon_{im}$$

I_{im} represents parental investments in child i in family m, e_{im} is a dummy variable that represents child i's health endowment (equals 1 if the child has poor health and 0 otherwise), X_{im} is a vector of child-specific control variables, τ_m controls for family-specific unobserved factors, and ε_{im} is the error term. Controls include age of the child, sex of the child, designated parent's health at birth, designated parent's education at birth, and mother's age at time of birth.

The coefficient of interest is β_1, which represents the effect of poor health endowment in one sibling on parental investment. A negative value for β_1 would indicate reinforcing behavior whereas a positive value for β_1 would indicate compensatory behavior. To see if I get the same effects as in Hsin (2012), I also estimate the effect of health endowment on investment by maternal education level,

[29] This was done to facilitate analysis because of the way the outcomes were recorded in the SIPP. While the other outcomes were measured as the number of times over the last week or the number of times over the past month, for these outcomes, the outcomes were measured in categories of frequency (never, sometimes, etc).

where I define less educated mothers as high school graduate or less and more educated mothers as having more than a high school education (some college education or more).

4. RESULTS

Sample sizes for each of the age groups are presented in Table 3.3 by whether all the siblings in a family in that age group were all of poor health, were all of good health, or were of different health endowments. The number of sibling groups in the last group is, of course, particularly important because that is where the variation of interest is coming from. In the age 0 to 6 group, there were 32 sibling groups where all had poor health, 10,891 sibling groups where all had good health, and 306 sibling groups where the siblings had different health endowments.[30] In the age 6 to 11 group, the sample was larger with 151 sibling groups where all had poor health, 14,601 sibling groups where all had good health, and 773 sibling groups where there was variation in health endowment. The age 6 to 14 group includes the age 6 to 11 age group, so the sample sizes are similar, but a bit larger.

Table 3.4 presents some summary statistics for each age group - 0 to 6, 6 to 11, and 6 to 14 - by health endowment and for the group overall. For each of the age groups, sample size of the good health group is much larger than the sample size for the poor health group as would be expected. Also, across all of the age groups, both the average age of the child and the average age of the mother at the time of birth as well as the average number of siblings in both the age group and in the family as a whole does not differ much depending on the health endowment of the child.

There are several differences between the children with poor health endowment and children with good health endowment. The poor health sample in

[30] These numbers are for all six panels. The numbers for just the four 1996-2008 panels are similar.

TABLE 3.3: NUMBER OF INDIVIDUALS AND SIBLING GROUPS IN EACH AGE GROUP

		All Panels	1996-2008 Panels		
All Siblings Low Health Endowment	# Siblings Groups	32	31	151	218
	# Individuals	134	127	493	738
All Siblings High Health Endowment	# Siblings Groups	10,891	10,058	6,473	7,419
	# Individuals	24,584	22,845	14,601	17,315
Different Health Endowments	# Siblings Groups	306	275	773	941
	# Individuals	697	632	1,773	2,287

TABLE 3.4: SUMMARY STATISTICS

	AGE GROUP 0-6			**AGE GROUP 6-11**			**AGE GROUP 6-14**		
	ALL	BAD HEALTH	GOOD HEALTH	ALL	BAD HEALTH	GOOD HEALTH	ALL	BAD HEALTH	GOOD HEALTH
Number of Observations	*25,415*	*642*	*24,773*	*16,867*	*1,692*	*15,175*	*20,340*	*2,097*	*18,243*
Average Age	2.51	2.91	2.50	8.43	8.51	8.42	9.09	9.25	9.07
Percent Male	50.4%	59.2%	50.4%	50.2%	66.4%	48.4%	50.4%	66.3%	48.5%
Num. of Siblings in Age Group	1.70	1.75	1.70	1.71	1.74	1.71	1.79	1.82	1.79
Total Num. of Siblings	2.88	2.84	2.88	2.93	2.95	2.93	2.96	2.99	2.97
Mother's Age at Birth	29.5	29.5	29.5	29.0	28.9	29.0	28.8	28.7	28.8
Designated Parent: Disabled	4.8%	16.2%	4.5%	7.7%	15.9%	6.8%	8.2%	17.5%	7.1%
Designated Parent: HS Grad	26.8%	27.7%	26.8%	26.6%	26.9%	26.5%	26.6%	26.9%	26.6%
Designated Parent: College	16.8%	13.6%	16.9%	16.5%	12.4%	17.0%	16.1%	12.4%	16.5%
Designated Parent: Post-College	7.0%	3.6%	7.1%	6.5%	5.4%	6.7%	6.6%	5.6%	6.7%
Designated Parent: Married	76.3%	60.1%	76.7%	73.9%	65.4%	74.8%	73.8%	65.7%	74.8%
Total Household Income	$18,920	$15,263	$19,015	$21,114	$17,698	$21,495	$21,139	$17,848	$21,517

each of the age groups appears to be much more male-dominated. Also, the children in the poor health sample are much more likely to have a designated parent that is also disabled, which is to be expected. In terms of the socio-economic conditions, the designated parent of children in poor health tend to be better educated, have higher total household income, and are more likely to be married in all three of the age groups.

Figures 3.1 through 3.4 show the distributions for some of the outcome variables for each of the age groups where analysis was done using that outcome variable. In figure 3.1, which shows the distribution for the number of outings the child was taken on last week, the bulk of observations for both age groups are in the 0 to 15 range, but there are also spikes at 20, 25, and 30. Figure 3.2 shows a similar pattern for the number of times the child was read to in total last week, but without the spikes after the bulk of observations in the 0 to 10 range. Figure 3.3 shows that, for both age groups, while there is variance in the number of days last week that the designated parents ate breakfast with the child, there is a spike at 7 days. Lastly, figure 4 shows that, for the frequency with which the designated parent played with the child, while about half of the observations are above and below the average for the age 6 to 14 age group, the age 0 to 6 age group is much more skewed with more observations above average than below.

Table 3.5 presents the regression results from estimating the family fixed effect models for the sample in the age 0 to 6 group. In this table, as well as Tables 3.6 and 3.7, the results are divided into three panels with the top panel for all observations, the second panel for the subsample of children in families where the designated parent has a high school education or less, and the third panel for the subsample of children in families where the designated parents has at least some college education. For the pooled age 0 to 6 sample, the coefficient of interest is insignificant for all of the

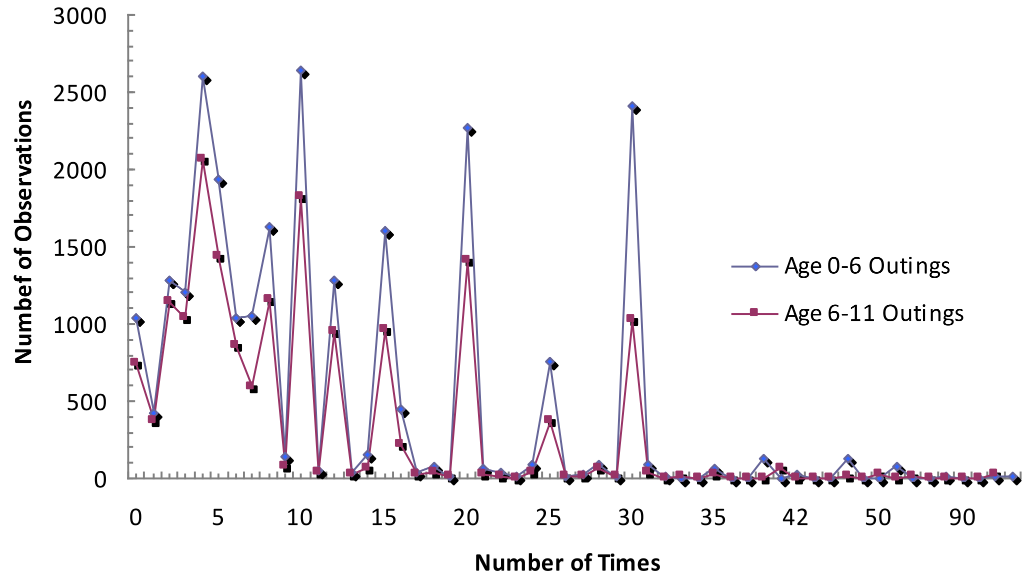

FIGURE 3.1: DISTRIBUTION OF OUTCOMES: NUMBER OF OUTINGS LAST WEEK

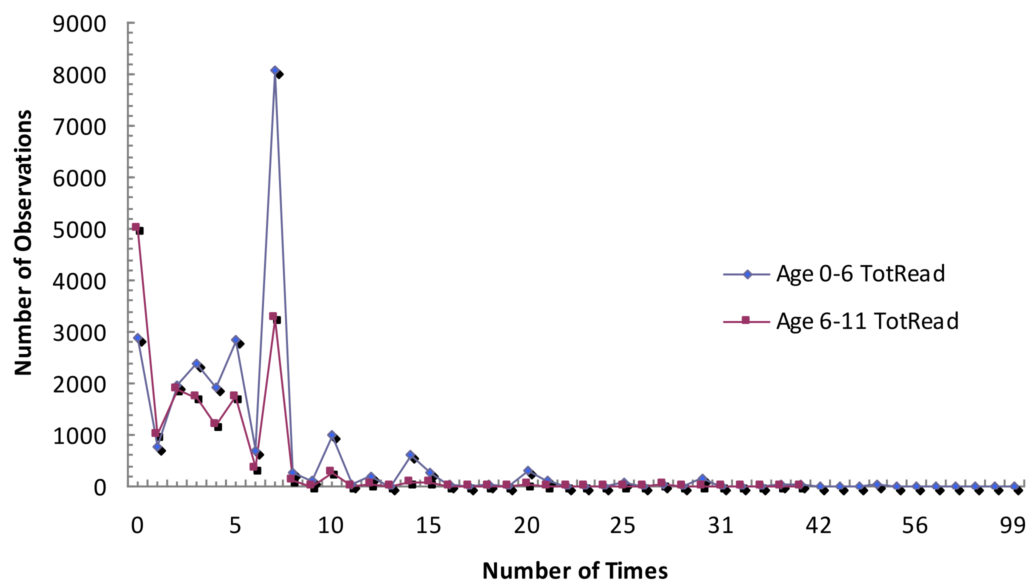

FIGURE 3.2: DISTRIBUTION OF OUTCOMES: NUMBER OF TIMES READ TO LAST WEEK

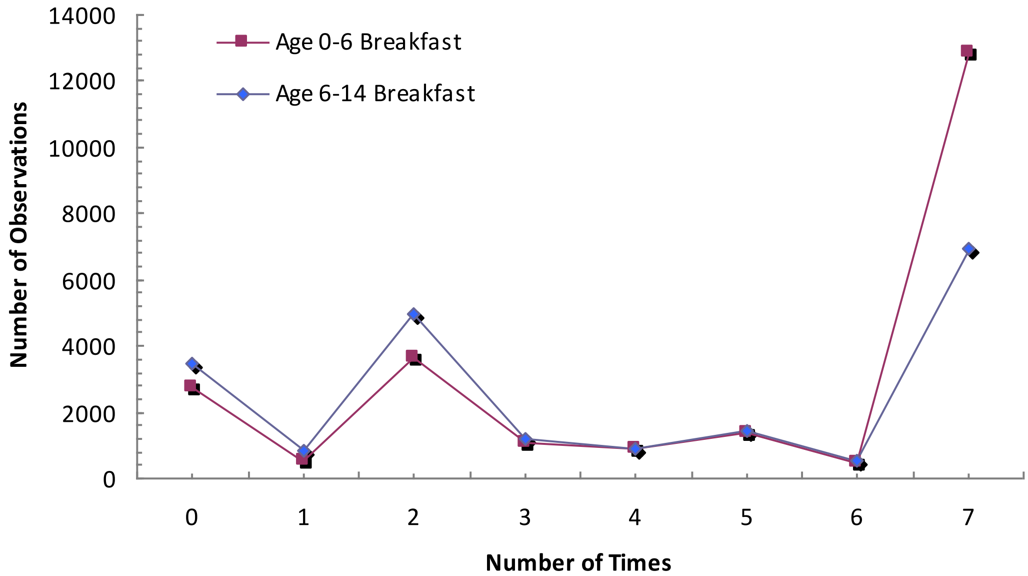

FIGURE 3.3: DISTRIBUTION OF OUTCOMES: NUMBER OF DAYS LAST WEEK ATE BREAKFAST WITH

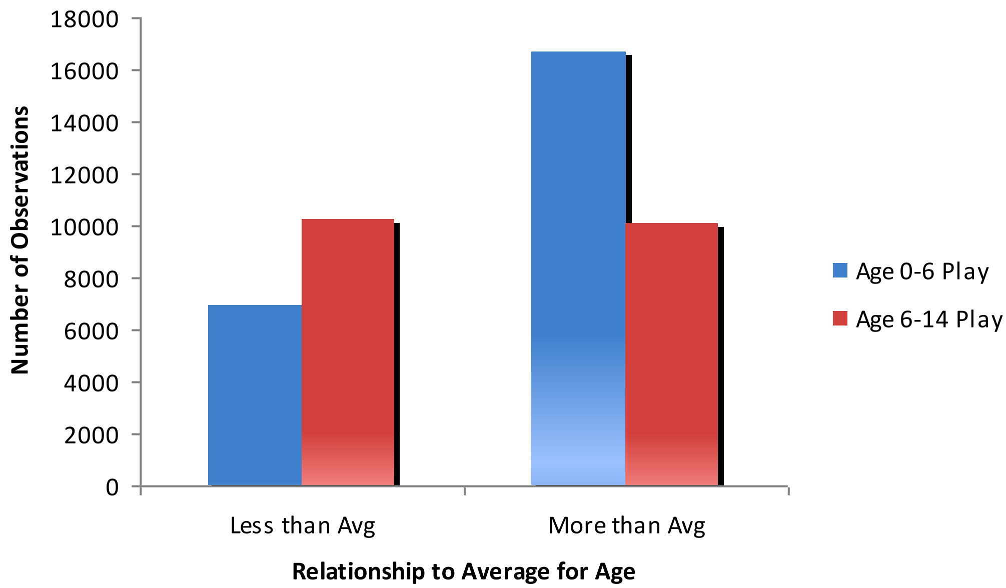

FIGURE 3.4: DISTRIBUTION OF OUTCOMES: FREQUENCY PLAYED WITH (ABOVE/BELOW AVERAGE)

TABLE 3.5: REGRESSION RESULTS FOR AGE GROUP 0-6

	Read: Total	Read: Designated Parent	Read: Dad	Outing	Eat Breakfast: Total	Eat Breakfast: Dad	Eat Dinner: Total	Eat Dinner: Dad	Fun Times: Total	Fun Times: Dad	Praise: Total	Praise: Dad
ALL OBSERVATIONS												
Bad Health	0.157	0.201*	0.0154	-0.0569	0.0946	0.141	-0.139	-0.0799	0.0279	0.0326	-0.0267	0.0994
	(1.375)	(1.937)	(0.0992)	(-0.781)	(0.847)	(1.445)	(-1.383)	(-0.691)	(0.148)	(0.232)	(-0.186)	(0.665)
Controls												
N	25008	22069	17120	24983	23522	18133	23522	18133	23522	18133	23522	18133
LOW EDUCATION												
Bad Health	0.141*	0.296**	0.0843	-0.197**	0.0465	0.297**	-0.101	-0.0318	-0.0422	0.0473	-0.0570	0.00791
	(1.709)	(2.408)	(0.779)	(-2.149)	(0.253)	(2.204)	(-0.590)	(-0.228)	(-0.132)	(0.233)	(-0.288)	(0.0359)
Controls												
N	10790	9107	6236	10775	10159	6878	10159	6878	10159	6878	10159	6878
HIGH EDUCATION												
Bad Health	-0.153	-0.147	-0.289*	0.129*	-0.0261	-0.0855	-0.231**	-0.191	0.0424	0.00513	-0.0623	0.125
	(-1.298)	(-1.633)	(-1.813)	(1.781)	(-0.231)	(-0.707)	(-1.999)	(-1.258)	(0.195)	(0.0282)	(-0.350)	(0.637)
Controls												
N	14218	12816	10803	14208	13363	11163	13363	11163	13363	11163	13363	11163

Notes: This table shows the coefficient of interest (on an indicator variable for bad health) for each regression equation, where the outcome variable is shown at the top of each column. The top panel shows the results for all observations, the middle panel is for families where the designated parent has a high school education or less), and the bottom panel is for families where the designated parent has at least some college education. Robust standard errors in parenthesis; * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level. Controls include age of the child, sex of the child, designated parent's health at birth, designated parent's education at birth, and mother’s age at time of birth.

investments except for “Read: Designated Parent,” which is how many times the child was read to by the designated parents. For this investment, β_1 is only marginally significant at the 10% level and is positive. This result provides evidence that for reading parents may be compensating for initial differences in health endowment, but there is no evidence that for the other outcomes, parents are reinforcing or compensation for initial differences and the sign of the insignificant coefficients are neither all positive nor all negative.

Turning to the low education age 0 to 6 sample, β_1 is positive and significant for the specifications with “Read: Designated Parent,” “Read: Total” and “Eat Breakfast: Dad” (how often the dad ate breakfast with the child) as investments, indicating compensating behavior among less-educated parents for these outcomes.

In contrast, β_1 is negative and significant for “Outing” (how many times the child was taken on fun outings). In all three panels in Table3.5, the sign of β_1 for “Outings” is opposite to that for the reading investments, supporting the general result that the specific investment that is considered matters when determining whether parents reinforce or compensate. This makes sense since, for example, reading to a child and taking a child on outings are very different in nature. In the case of outings, parents may be reluctant or less able to take children who are sicker on outings whereas they are less restricted in their ability to read to children of poor health. These differences in restrictions may well vary by socioeconomic class as parents with more resources may be more able to overcome the restrictions.

In contrast to the results for the low education group for the reading investments, for the high education age 0 to 6 sample, β_1 is negative and significant for “Read: Dad” (number of times child was read to by the dad), indicating reinforcing behavior. β_1 is also negative and significant for “Eat Dinner: Total” (total number of times parents at dinner with the child), again indicating reinforcing behavior. Lastly, as touched on before, the coefficient of interest is of the opposite sign (positive) and significant for "Outing."

Altogether, the results in Table 3.5 for the younger age 0 to 6 sample provide some evidence that on average, in terms of reading to the child, parents exhibit compensatory behavior, but that highly educated parents engage in reinforcing behavior. This would directly contrast the theory in Conley (2008) and the results in Hsin (2012). There is also some suggestive evidence that less educated parents exhibit a different pattern of behavior from highly educated parents for outings and eating meals together, but it is important to remember this evidence is suggestive at best since many of the coefficients fail to attain statistical significance.

The first set of regression results for the older set of children, the age 6 to 11 age group, are in Table 3.6. Here, only four of the investments were considered (see Table 3.2) as the results for the rest of the investments are estimated on the age 6 to 14 age sample. For the pooled 6 to 11 sample, β_1 is not significant for any of the investments.

Turning to the middle panel, the results for children with less educated parents are more interesting. β_1 is positive and significant for both "Read: Total" (total number of time the child was to by anyone) and "Read: Designated Parent," (the total number of times the child was read to by the designated parent) indicating compensating behavior among parents. This is consistent with the results on reading for the age 0 to 6 sample. For the other reading investment variable "Read: Dad," β_1 is also positive, but not significant and for "Outing," β_1 is insignificant and negative.

Interestingly, results in the bottom panel for children with highly educated parents shows that β_1 is positive and significant at the 10% level for "Read: Designated Parent." This indicates compensating behavior, the same as for the less educated group in this sample. This pattern of results is different that for the age 0 to

TABLE 3.6: RESULTS FOR AGE GROUP 6-11

	Read: Total	Read: Designated Parent	Read: Dad	Outing
		ALL OBSERVATIONS		
Bad Health	0.0639	0.0106	-0.0714	-0.0310
	(1.056)	(0.170)	(-0.704)	(-0.674)
Controls				
N	16834	14020	10134	16834
		LOW EDUCATION		
Bad Health	0.198**	0.155*	0.175	-0.00983
	(2.034)	(1.935)	(1.110)	(-0.258)
Controls				
N	7049	5529	3582	7049
		HIGH EDUCATION		
Bad Health	0.0947	0.107*	0.0388	-0.00751
	(1.468)	(1.764)	(0.514)	(-0.118)
Controls				
N	9785	8311	6451	9785

Notes: This table shows the coefficient of interest (on an indicator variable for bad health) for each regression equation, where the outcome variable is shown at the top of each column. The top panel shows the results for all observations, the middle panel is for families where the designated parent has a high school education or less), and the bottom panel is for families where the designated parent has at least some college education. Robust standard errors in parenthesis; * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level. Controls include age of the child, sex of the child, designated parent's health at birth, designated parent's education at birth, and mother's age at time of birth.

6 age group, where the coefficient of interest on the reading outcomes were of the opposite signs for the less educated and more educated groups.

Table 3.7 has the results for the age 6 to 14 age group. Here the rest of the investments that were not used for the age 6 to 11 age group are considered. In the pooled age 6 to 14 sample, β_1 is negative and significant for “Eat Breakfast: Dad” (how often the dad after breakfast with the child), which indicates reinforcing behavior among these parents for this outcome. Interestingly, the point estimate on this coefficient was positive, but insignificant for the age 0 to 6 pooled sample, which means there is no evidence of either compensation or reinforcement in that age group. This, along with the reading results for high education samples, indicates that the pattern of behavior among parents may also depend on the age range of the siblings.

For the age 6 to 14 low education sample, β_1 is negative (reinforcing) and significant for “Fun Times: Dad" (how often the dad played with the child just for fun) and “Praise: Dad.” These same coefficients were insignificant (and positive) for the age 0 to 6 age group. Also, while “Eat Breakfast: Dad” was positive and significant for the age 0 to 6 group, it is insignificant (and negative) for the age 6 to 14 age group.

For the age 6 to 14 high education sample, the coefficient of interest is positive and significant for “Praise: Total” indicating compensating behavior. This is different from the age 0 to 6 sample, where this coefficient was insignificant (and negative). In addition, “Eat Dinner: Total” was negative and significant for that sample, but is insignificant (and positive) here. These results are consistent with the above results indicating that parental behavior may differ for older siblings versus younger siblings.

Taken altogether, the results for the older sample, age 6 to 11 or 14 suggest that less educated parents may exhibit compensating behavior in terms of the reading related investments and hints of reinforcing behavior in terms of the some of the other investments (especially, praise and playing together). There are also hints of compensating behavior among highly educated parents in terms of reading and praise.

TABLE 3.7: RESULTS FOR AGE GROUP 6-14								
	Eat Breakfast: Total	Eat Breakfast: Dad	Eat Dinner: Total	Eat Dinner: Dad	Fun Times: Total	Fun Times: Dad	Praise: Total	Praise: Dad
				ALL OBSERVATIONS				
Bad Health	-0.0126	-0.0864**	0.0381	-0.0661	0.0598	-0.0933	0.00668	-0.105
	(-0.379)	(-1.969)	(0.937)	(-1.616)	(0.910)	(-1.150)	(0.106)	(-1.253)
Controls								
N	20301	14593	20301	14593	20301	14593	20301	14593
				LOW EDUCATION				
Bad Health	0.0282	-0.0398	0.0480	-0.0577	0.0654	-0.225*	-0.108	-0.285**
	(0.633)	(-0.458)	(0.754)	(-0.957)	(0.805)	(-1.927)	(-1.208)	(-2.307)
Controls								
N	8632	5495	8632	5495	8632	5495	8632	5495
				HIGH EDUCATION				
Bad Health	0.0565	-0.0563	0.0496	0.0183	0.143	0.0490	0.158**	0.0927
	(1.479)	(-1.256)	(1.073)	(0.404)	(1.536)	(0.471)	(1.962)	(0.865)
Controls								
N	11669	8934	11669	8934	11669	8934	11669	8934

Notes: This table shows the coefficient of interest (on an indicator variable for bad health) for each regression equation, where the outcome variable is shown at the top of each column. The top panel shows the results for all observations, the middle panel is for families where the designated parent has a high school education or less), and the bottom panel is for families where the designated parent has at least some college education. Robust standard errors in parenthesis; * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level. Controls include age of the child, sex of the child, designated parent's health at birth, designated parent's education at birth, and mother’s age at time of birth.

In general, however, with so many insignificant coefficients, the results taken altogether seem to show that, the specific investment that is being considered is of critical importance and there is no evidence of reinforcing or compensating behavior among parents for the bulk of the investments considered. The results also suggest that parents may invest differently for older children versus younger children.

4.1 Robustness checks

One might expect that the results depend critically on how the bad health variable is created for each of the age groups. As robustness checks, for the age 6 to 11 and the age 6 to 14 age groups, I estimated the results using different variations of the bad health variable. I did not do the same robustness check for the age 0 to 6 age group because, depending the SIPP panel, there were only one or two questions used to create the variable and, thus, little room for variation. For the older age groups, the results were qualitatively unchanged after changing the bad health variable in the following ways: requiring a "yes" to at least two and at least three of the questions in Table 3.1 rather than just one, defining bad health as having one of the health conditions in Table 3.1, defining bad health as having one of the difficulties in Table 3.1, and defining bad health as requiring the use of one of the aids in Table 3.1.

I also performed the same analysis without age adjusting the investment outcomes for each child (described in the data section). These results are shown in Tables 3.8, 3.9, and 3.10 (analogous to Tables 3.5, 3.6, and 3.7). Generally speaking, these results look very similar to the results using age-standardized measures of parental investment and qualitatively speaking, the conclusions are largely unchanged.

TABLE 3.8: RESULTS FOR AGE GROUP 0-6 (Not Adjusted)

	Read: Total	Read: Designated Parent	Read: Dad	Outing	Eat Breakfast: Total	Eat Breakfast: Dad	Eat Dinner: Total	Eat Dinner: Dad	Fun Times: Total	Fun Times: Dad	Praise: Total	Praise: Dad
						ALL OBSERVATIONS						
Bad Health	0.0936	0.0818	-0.114	-0.0346	0.0429	0.0961	-0.0114	-0.0155	0.0519	0.0325	0.00875	0.0996
	(1.333)	(1.463)	(-0.991)	(-0.676)	(1.027)	(1.588)	(-0.540)	(-0.467)	(0.271)	(0.232)	(0.0610)	(0.668)
Controls												
N	25008	22069	17120	24983	23522	18133	23522	18133	25008	18133	25008	18133
						LOW EDUCATION						
Bad Health	0.187**	0.181**	-0.00329	-0.0891	0.0192	0.210**	-0.00692	-0.0315	-0.0124	0.0476	-0.0147	0.00834
	(2.185)	(2.563)	(-0.0274)	(-1.299)	(0.269)	(2.230)	(-0.163)	(-0.658)	(-0.0391)	(0.234)	(-0.0766)	(0.0379)
Controls												
N	10790	9107	6236	10775	10159	6878	10159	6878	10790	6878	10790	6878
Bad Health	-0.101	-0.0900*	-0.296**	0.0478	0.00147	-0.0664	-0.0262	-0.0364	0.0494	0.00332	-0.0433	0.125
	(-1.373)	(-1.718)	(-2.374)	(1.050)	(0.0334)	(-0.812)	(-1.134)	(-0.804)	(0.226)	(0.0183)	(-0.241)	(0.634)
Controls												
N	14218	12816	10803	14208	13363	11163	13363	11163	14218	11163	14218	11163

Notes: This table shows the coefficient of interest (on an indicator variable for bad health) for each regression equation, where the outcome variable is shown at the top of each column. The top panel shows the results for all observations, the middle panel is for families where the designated parent has a high school education or less), and the bottom panel is for families where the designated parent has at least some college education. Robust standard errors in parenthesis; * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level. Controls include age of the child, sex of the child, designated parent's health at birth, designated parent's education at birth, and mother’s age at time of birth.

TABLE 3.9: RESULTS FOR AGE GROUP 6-11 (Not Adjusted)

	Read: Total	Read: Designated Parent	Read: Dad	Outing
		ALL OBSERVATIONS		
Bad Health	0.0777*	0.0117	-0.0487	-0.00140
	(1.658)	(0.275)	(-0.505)	(-0.0395)
Controls				
N	16834	14020	10134	16834
		LOW EDUCATION		
Bad Health	0.203**	0.0790	0.222	0.0173
	(2.508)	(1.482)	(1.201)	(0.486)
Controls				
N	7049	5529	3582	7049
		HIGH EDUCATION		
Bad Health	0.0507	0.0576	-0.0181	0.0111
	(1.200)	(1.298)	(-0.249)	(0.250)
Controls				
N	9785	8311	6451	9785

Notes: This table shows the coefficient of interest (on an indicator variable for bad health) for each regression equation, where the outcome variable is shown at the top of each column. The top panel shows the results for all observations, the middle panel is for families where the designated parent has a high school education or less), and the bottom panel is for families where the designated parent has at least some college education. Robust standard errors in parenthesis; * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level. Controls include age of the child, sex of the child, designated parent's health at birth, designated parent's education at birth, and mother’s age at time of birth.

TABLE 3.10: RESULTS FOR AGE GROUP 6-14 (Not Adjusted)

	Eat Breakfast: Total	Eat Breakfast: Dad	Eat Dinner: Total	Eat Dinner: Dad	Fun Times: Total	Fun Times: Dad	Praise: Total	Praise: Dad
				ALL OBSERVATIONS				
Bad Health	-0.0442**	-0.0807**	0.00784	-0.0155	0.0598	-0.0854	-0.00210	-0.0925
	(-2.056)	(-2.293)	(0.868)	(-1.181)	(0.910)	(-1.072)	(-0.0325)	(-1.265)
Controls								
N	20301	14593	20301	14593	20301	14593	14593	14593
				LOW EDUCATION				
Bad Health	-0.0104	-0.0502	0.00914	-0.0127	0.0647	-0.186*	-0.162*	-0.277**
	(-0.356)	(-0.655)	(0.682)	(-0.645)	(0.798)	(-1.660)	(-1.783)	(-2.466)
Controls								
N	8632	5495	8632	5495	8632	5495	8632	5495
				HIGH EDUCATION				
Bad Health	0.00365	-0.0485	0.0135	0.00649	0.141	0.0347	0.182**	0.0758
	(0.159)	(-1.381)	(1.236)	(0.456)	(1.513)	(0.358)	(2.212)	(0.924)
Controls								
N	11669	8934	11669	8934	11669	8934	11669	8934

Notes: This table shows the coefficient of interest (on an indicator variable for bad health) for each regression equation, where the outcome variable is shown at the top of each column. The top panel shows the results for all observations, the middle panel is for families where the designated parent has a high school education or less), and the bottom panel is for families where the designated parent has at least some college education. Robust standard errors in parenthesis; * significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level. Controls include age of the child, sex of the child, designated parent's health at birth, designated parent's education at birth, and mother’s age at time of birth.

5. DISCUSSION

The question of how parents allocate resources between children of different endowments has long been of interest in the economics literature. Theories that predict reinforcing behavior and theories that predict compensating behavior by parents both exist. On the one hand, parents may want to invest where the marginal return would be highest, which would be in the more endowed child (Becker and Tomes, 1976 and 1986). However, parents may also be driven equity concerns and wish to equalize outcomes across all of their children, leading to greater investment in the lesser-endowed child (Behrman et al., 1982). Which of these motivations dominate may very well depend on the socio-economic status of the family. In this context, only wealthier families are able to "afford" the luxury of equality in outcome whereas poorer families are better off ensuring that at least one child achieves some minimal acceptable outcome and, thus, pursue the strategy of investing where the marginal returns are the highest (Conley, 2008; Hsin, 2012).

In contrast to the theories predicting either reinforcing or compensating behavior, the health economics literature looking at the long run effects of childhood health has made frequent use of sibling fixed effect models assuming that parents invest equally in children of differing health endowments. As Almond and Currie (2011) discuss in their recent Handbook of Health Economics chapter, if parents compensate for initial health endowments, then those fixed-effects estimates would understate or serve as a lower bound of the true effects, but if parents reinforce initial differences, then the fixed effects estimates would be more problematic because they would represent the combination of the true underlying effect and the effect of parental reaction to differences in endowment.

The previous empirical literature on this question finds evidence of all of the possible patterns of behavior – neutral, reinforcement, and compensation. The older studies using indirect measures of both initial endowments and parental investments have found both that parents compensate (Griliches, 1979; Behrman et al, 1982) and that parents reinforce initial differences (Behrman et al., 1994). More recent studies have used more direct measures of both initial health endowment and parental investment. Several of these have found no evidence of either compensating or reinforcing behavior (Del Bono et al, 2008; Royer, 2009; Kelly, 2009; Almond and Currie, 2011) while Datar et al (2010) found that parents reinforce and Hsin (2012) finds that less educated parents reinforce while more educated parents compensate. Studies of developing countries have generally found evidence of reinforcing behavior (Ayalew 2005; Pitt et al. 1990; Rosenzweig and Schultz 1982; Rosenzweig and Wolpin 1988).

The results from this study are not consistent with any of the previous studies in isolation, but are consistent with the literature as a whole. There are a few main takeaways. First, I find some evidence that parents do not invest equally in children of different health endowments. As discussed, this indicates that sibling fixed effect studies of childhood health conditions on long-term outcomes may be over- or under-stating the true effects.

However, most of the investment outcomes I looked at yield insignificant estimates of the coefficient of interest, providing no evidence of either reinforcing or compensating behavior. In addition, the actual direction of effect, whether parents compensate or reinforce initial differences in endowment is unclear in this study. Instead, the pattern (reinforcement or compensation) seems to depend on the specific investment being considered. My results as a whole look similar to the previous literature taken altogether and sheds some light on how to interpret results from previous similar empirical studies, which all use very specific investments and offer seemingly contradictory results. These results would indicate that the reason for the disparity in previous estimates is the heterogeneity in the specific set of investments that are being considered in each study. This should not be surprising. Some of the investments used in this and previous studies may inherently be limited in that less healthy children may naturally be less able or likely to partake in spite of whatever their parents' intentions are. Examples include outings and well-baby visits.

My results also offer some evidence that parents' behavior may vary across the children's age group (age 0 to 6 versus age 6 to 11/14) as well as parents' education level (those with a high school education or less versus those with at least some college education). Of course, it is important to note that a weakness of this study is that I have no way of knowing whether the heterogeneity in results across age groups is the result of the children's age group or how I am measuring health endowment in each age group. Nonetheless, the results in this study show that parental behavior in response to children with different health endowments is very context dependent and there may not be a simple answer.

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